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OF SCIENCES; OF THE ROYAL SOCIETY OF SCIENCES OF DENMARK;
OF THE ROYAL SOCIETY OF GÖTTINGEN, &c. &c.

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ERRATA.

- Vol. v. p. 28, line 3, for $p = 1 + c g$, read $p = 1 + e g$.
 — p. 215, line 14, for $p = 1 e + g$, read $p = 1 + e g$.
 — p. 333, last line, for family read group.

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No. XIII.

Of the NEW SERIES will be published on the 1st of JULY 1832.

THE
EDINBURGH
JOURNAL OF SCIENCE.

ART. I.—*First Meeting of the British Association for the Advancement of Science, held at York in September 1831.* By JAMES F. W. JOHNSTON, A. M. &c. &c. Communicated by the Author.

THE *Great Scientific Meeting* at York has taken place, and it has exceeded the most sanguine expectations of its projectors. It had been spoken against,—it had been written against,—private feelings had been awakened against it,—public bodies had regarded it with jealousy, and their members had deemed it necessary to refuse it their countenance,—yea, some decidedly scientific men, who have illustrated their own departments, and of whom their country has reason to be proud, had been induced to set their faces against it;—yet in spite of all these hindrances, the experiment of a great national meeting has been tried, and it has succeeded. The foundation of a general national institution has been laid, which, fixed to no spot, is free to range from city to city of this great empire, gathering into its stores the genius and information of every district, awakening men, wherever it bends its footsteps, to the dignity and importance of science, and scattering into every corner as it passes through the land some new seeds of valuable discovery, which duly fostered may ripen into a harvest of resources hitherto not known, and therefore undeveloped,—an institution which, limited to no science, can comprehend within its

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ample bounds the votaries of every branch of natural knowledge, ready each, and willing to eliminate by the conjoint researches of all, those complicated mysteries of nature which the most ardent philosophers are ever meeting with in their single and isolated investigations, and which even the united efforts of all the cultivators of any one department could never have revealed. Modern science has indeed for some time demanded the establishment of such an association. For the limits of many branches of knowledge which men a few years back could define and clearly explain the scope of, so as to draw a distinct line between them and the other departments bordering most closely upon them,—have now been pushed forward so far as to have overpassed each other, and intermingled their mutual learning; and thus, instead of so many separate and minor fields of investigation as in former days, the philosopher can see now but one wide field of universal science, wherein the fruits of each department are seen growing and intermingling with each other, and stretching over so vast a space as to baffle the most enlarged intellect to embrace it. But what one single mind or one single branch of study cannot effect, the united efforts of many may achieve; and while the chemist, and the botanist, and the geologist combine with the mathematician and the physical philosopher, to unravel the same complex and interwoven relations of nature, we need not despair of seeing a light hitherto un hoped for scattered over her most extensive and most hidden operations. The truth of this view of the present state of human knowledge, and the method of cultivation it calls for, the more it is thought of will, I am convinced, become the more apparent. And when the political excitement of this period has passed away, and men's minds have settled down again to that quiet and thinking state which is most necessary to the study of nature, they will recognize in the British Association an engine of efficacy and power sufficient not only to arrest the decline of science, but to achieve more numerous conquests in philosophy than Britain has yet to boast of.

Were there then no advantages to result to men of science from mutual and personal intercourse—were there no little feelings of jealousy to be removed—no controversial disputes

or scientific differences to be made up—no misconceptions to be cleared away—no false estimates of the talents or temper of our fellow labourers to be corrected—no gratification to be derived from seeing and conversing with them ;—and were no increase of knowledge to be acquired—no new facts more speedily and better learned and understood—and no new views elicited by mutual communion and discussion among men of the same science ;—were no new lights to be struck out by these discussions—and no new subjects of inquiry suggested and recommended,—still there remains in the view now given of the present state of science a sound and sufficient, if not an imperative, reason for the establishment of such a general institution as the British Association professes to be. There is a new, a fertile, and a boundless field of mutual co-operation spread out before it. The botanist meets with a chemical problem in his researches into the physiology of plants, he refers it to his chemical associates, and by them it is solved. The chemist obtains a series of experimental results, which, put into the hands of the physical section, may form the germ of a new branch of physics. The zoologist, the geologist, the mineralogist, and the chemist, are so linked together, that they cannot go far without being either necessitated to seek, or being benefited by obtaining the suggestions of each other. Thus all are mutually dependent ; and whatever tends to cement the union of the sciences, and to bring their votaries into more frequent and more intimate communion, must do much towards facilitating the progress of knowledge. On this ground alone, then, of professing to bring men of all sciences together, might the British Association rest its claim to general approbation and support.

The council of the Philosophical Society of York deserve well of scientific men for the readiness with which they came forward to superintend the arrangements for this general meeting, as well as for the zeal and spirit they manifested throughout the entire week of the assembly. It was desirable that the first meeting should be thus supported by the exertions of active and zealous men, both that the new association might be built upon a sure foundation, and that future committees in

other cities might have an example at once to stimulate and direct their preparations.

The choice of York as the first place of meeting has therefore proved a happy one. It was suggested chiefly by its locality as a most central city, and the advantage it possessed in a prosperous and well arranged Philosophical Society, together with the assistance voluntarily offered by its managers, confirmed the choice. The York Society, under the superintendence of their talented vice-president, the Reverend William Vernon Harcourt, and their amiable and indefatigable secretary Mr Phillips, have amply fulfilled the most enlarged expectations, and Oxford has the harder task to perform that York has gone before it.

The people of York have a right to know something of science; for besides men of talent devoted to other pursuits, they have among them an able geologist, nephew and follower of the father of English geology;—and they may claim some praise as promoters of science, for they have erected in honour of it the largest and most splendid building of which any provincial philosophical society can boast. In this building the meetings were held, and its general design—its theatre, its committee rooms, the suite of apartments forming the museum, and the admirable condition, and still more admirable arrangement, of the infant collections they contained, were subjects of general approbation. The collection of organic remains is the most extensive; and it is fitting that Yorkshire, which has proved so rich a storehouse of extinct animal remains, should also take some pains to display the treasures she has so long and so carefully preserved within her bosom; and that, since the time has now come when the secrets of nature may be usefully revealed to inquiring men, she may have the credit, not only of well preserving, but also of wisely and timeously revealing, what she had so long kept hid in prudent concealment.—The minerals are not so numerous, but they are in general good specimens, and hold out the promise of a cabinet which in due time, if past and present zeal continue, will render the York museum not unworthy of the attention of the mineralogist. There is also a respectable collection of birds and insects, and the germ of a scientific library, which

will give a proper direction and a new impulse to the spirit of inquiry, to which the natural history collections may give rise.

The Philosophical Society of York has been only a few years in existence, and its museum has been but a short time opened, and yet its collections are such as to excel those of many old and chartered institutions. Let their labours be continued, and they will benefit not themselves only, but the times in which we live; for it cannot be, that, though borne down with the load of years, even our most aged institutions should not at length bestir themselves, and, catching from memory a spark of their own young enthusiasm, should not again start forward in the race of improvement and amelioration, and assume that place among the promoters of science and the helpers and patrons of scientific men which they have left, and are still leaving to younger and more zealous institutions. The theory of a perfect society for the promotion of science, supposes that the most active and talented men should have most to say in the management of its affairs; but the practice has at the present day been reversed in regard at least to one of these qualities, and the most *inactive* men are often found filling the most important official situations. Men who have long ceased to do any thing for science, who do not even profess to patronize it,—who in a long lifetime may or may not have effected the discovery of a single fact,—and who, jealous of their little fame, cast themselves in the way of such as might outstrip them, have been permitted to retard the workings and impair the efficiency of well constituted societies; but if the spirit of the time do not now so pervade these institutions as to cause them to reform themselves, they will ultimately be forced into it by very shame, at the progress and exertions of the provincial societies.

Nor is it merely from what they might themselves do that activity in the council of a scientific body is desirable, but from what they might induce others to perform. Many who know not what they might do, nor what circumstances place in their power, may do much if properly directed; and who so proper to throw out hints and suggestions to such men, as those who hold offices in a society having solely or mainly in view the promotion of science? Who could better solicit, direct, or employ the patronage of wealthy men, friends of science, and willing to

contribute in means what they are unable to give in actual labour; but such a council, if able, active, and themselves known for their labours? And who so well as such a body could apply to government for aid, either direct or indirect, in the promotion of any scientific object, important either to the honour or to the prosperity of the country? Who can believe for instance, that, had the scientific societies of Edinburgh, been represented by such an active and stirring body, they would have been obliged to this day to pay to government a yearly rent* for the rooms which they occupy? As if the members of any enlightened government might not by such men have been *at length* persuaded, to how much better purpose this money might be employed in encouraging the labours of scientific men. To the committee of the British Association all the active and working men of the day have a right to belong; and though we cannot tell what age may effect in retarding or impairing the efficiency of its operations, the united labours of so many diligent and talented men must have a powerful, and, it is to be hoped, a highly beneficial effect on the science of Great Britain.

The scenes at the meeting of the men of science at York were neither so numerous nor so interesting as I had previously found them at Hamburgh.† Englishmen meet in quiet, and they rarely indulge in those forms of greeting which are common with our continental neighbours,—yet something striking might have been elicited even from English philosophers on their first coming together, had the arrangements of the York committee provided any place of general public rencounter. As it was, introductions or first meetings generally took place in private, and thus their moral and instructive effect was lost. Few things give us a better idea of the character of any individual, and of the general estimation in which he is held, than the manner in which we find him treated by others. Where respect is paid, we involuntarily pay respect also without inquiring why; and where we see one man of known talent deferring to, and tacitly acknowledging by his bearing, the superiority of another, we have obtained an important element towards the forming a true

* The rent, taxes, &c. paid by the Royal Society alone amount to about L. 40 for each sitting.

† See this Journal, No. viii. New Series, p. 189.

opinion of both. There is food for philosophy, therefore, in the *phenomena* of introductions which no thinking man will despise.

The first general meeting took place on the evening of Monday the 26th of September. It was a preparatory meeting, but so showy and glittering that a stranger might have thought men had here met together to turn philosophy into sport, rather than to cultivate "science in earnest." But it was only the first proof, of which we afterwards received many, of the kindly feelings and hospitality of the people of York, which had induced them on this occasion to assemble,—ladies and gentlemen with equal zeal,—to do honour to science, and by giving a cordial and enthusiastic welcome to its cultivators, to testify their high sense of its dignity and importance.

In the course of the evening Mr Phillips delivered a short and popular extempore lecture on some of the more remarkable geological phenomena of Yorkshire, and exhibited some very interesting specimens, chiefly of organic remains, found in different parts of the country. The most remarkable of these remains were the head and horns of a species of deer, probably the red deer, from Thorn Waste on the river Dunn, which were black, and in some places quite flexible like leather. The bogs, therefore, in which they had lain, had not only dissolved out the phosphate of lime, but had also tanned the gelatine of the bones.

On the following morning (Tuesday 27th) was held the first meeting of the lovers of science for the purpose of forming the intended association. It was highly gratifying to those who had taken an interest in getting up and in promoting the objects of the meeting, to find so numerous and respectable a body of men met together. On this first meeting the theatre was well filled, and, before two days had elapsed, the number of those who had enrolled their names and taken out tickets amounted to about 350.

The first proposition made to the meeting was by Dr Brewster, that Lord Milton be requested to take the chair. His Lordship, as President of the Yorkshire Philosophical Society, was the most proper person that could be chosen; and this opinion at that time unanimously expressed was afterwards confirmed when the society became organized, by his being elected

to the office of annual president of the British Association for the advancement of Science.

After returning thanks, his Lordship* proceeded to express his conviction of the utility of such meetings, and of the advantages that would result to science from the establishment of a general scientific association. "When he saw the form which the Yorkshire Philosophical Society had taken, and the establishments it had formed, when he recollected that it had not existed more than eight or ten years, when, (he believed he should not be incorrect in saying) its existence might be traced to those most curious discoveries that were made in their own neighbourhood, (the cave at Kirkdale,) when they might trace the existence of a society which was now so considerable to so inconsiderable a source, he hoped that he was not too sanguine in anticipating that the present meeting might lead to great advantages to science in this country. He hoped that it might be the means of impressing on the government of this country, that the love of science, and the means of promoting science, were not confined to the metropolis; and he also hoped that when government was fully impressed with a just notion of the desire that was entertained to promote science in every part of the empire, they would see clearly the advantage of giving encouragement to such societies, and by every proper means advancing the interests of science. He wished, however, not to be misunderstood. He conceived that the best mode, he might say the most English mode, in which government could promote the advance of science, was by removing all impediments to it, rather than by offering direct encouragement. Not that there were not some branches of science and discovery that must be conducted on so great a scale as to be beyond individual means, and to those branches of research it was perhaps desirable that the attention of the government should be directed. They all knew that there were many grievous obstacles to the advancement of science in the fiscal laws of the country. He would give an instance. He believed in the science of optics there were serious obstacles in consequence of the tax on glass: other persons would be able to give other instances. He thought no doubt

* In a speech reported at length in the York newspapers and in the Leeds Mercury.

could be entertained, that if the example set there, should be followed up in other parts of England, and if those persons who had assembled on the present occasion should give encouragement to similar assemblies in different and well-selected parts of the kingdom, in a very few years this result would be obtained,—that men of science, who were now spread in different places of this empire, would be enabled to meet one another, to compare their ideas, to communicate to each other the advances they might have respectively made in their own spheres, and, by making known to others the wants and deficiencies they had respectively felt, to give such an impulse to science as could not but be highly beneficial. It was not merely that men of science would thus be warmed and encouraged, but it would undoubtedly result from such periodical meetings that each person in the society would be enabled to direct his researches in those modes which would be most advantageous to science in general.”

It was a happy but deserved compliment to the zeal and exertions of the Reverend Mr Vernon Harcourt with which his Lordship concluded :

Si monumentum queritis circumspicite.

His Lordship thought it desirable that science should be patronized, and that the attention of government should be drawn to it ; but he objects to all direct encouragement. On this point he is at issue with many eminent friends and cultivators of the highest departments of science. If England has hitherto done any thing for science, it has been at the expense and by the sacrifices of individuals. The intellect of the country has overmastered the difficulties which beset it, and, despite of the want of encouragement, the little honour and the little emolument they brought, has persevered in scientific researches to the honour and distinction of England. And shall scientific men alone be supported through life by the delights of study and by their own proud consciousness, and shall they obtain none of those more substantial comforts which other men enjoy and value themselves in possessing. If science benefits a country,—if the discoveries of science enhance the prosperity of a country, by increasing its wealth or its resources,

why should those who make the discoveries be the only people to lose by them? They not only gain nothing, but while they lose their time, even the cost of instruments and materials for experimenting are seldom repaid to them. Shall science in the abstract be an honourable pursuit, and yet shall the same diligence which maketh rich in any other profession make a man poor only in this? And now that men are beginning to see how precarious it is to trust the scientific honour of the country to individual and unrewarded exertion—and when, by the loss of our greatest men, we are awakened to the consciousness of how little we did for them when alive, and how feeble were our efforts to secure a race of eminent successors after them—are we now only to remove a few obstacles and alter a few fiscal regulations, as if by taking off a few imposts we were as certain to secure a speedy importation of scientific zeal into the country, as we are of foreign produce into our markets by a similar procedure? Hindrances, indeed, ought to be removed, but direct encouragement ought also to be given,—to be given, however, with prudence, and in the way most likely to produce the greatest good. Such direct encouragement is indeed *un-English*, for England has never so distinguished herself; but it is time her mode of encouraging science were altered. Were the duty taken off glass, experimental trials might be made without the same liability to vexatious interference;—but would the price of optical instruments be in consequence reduced—or would the chemist be materially benefited? No chemist in his single researches need break five pounds worth of glass in a year, and what would be the paltry encouragement he would receive by the remission of the half, nay the whole of that sum? The true friend of science will abolish no tax for the mere sake of lightening the burdens of scientific men, where such abolition is not called for by the general interests of the country. But let him apply a *per centage* of the produce of the tax to the direct encouragement of science, through such channels as men of known scientific eminence may suggest, and then may some good fruits be looked for.

It is said by popular men, who know no more of science than what the titles of patents teach them, and who judge of

the national encouragement of science from the splendid fortunes which a few happy applications of it to the arts have realized, that in this country science is its own reward, and that it needs no other aid than what is given it by a liberal and enlightened public. There are certain facts or discoveries, which, being of immediate application to the wants or luxuries of man, have an immediate commercial value—and for these the discoverer will in this commercial country always meet with a ready demand—and if he can keep his own secret, as Wollaston did, or *prevent his patent from being infringed*—may make it a source of great gain. Yet even to such discoverers the public is neither liberal nor patronizing—they deal with them as with merchants who have goods to sell, and the fruits of their researches are considered only as matters of traffic. And such discoveries as these do not constitute a hundredth part of the results of scientific investigation, and for the other ninety-nine there is in England no encouragement whatever.

How, then, is direct encouragement to be given? To answer this generally would be a difficult task; yet it is easy to see how in certain cases it might be afforded. Let the various boards, and similar establishments to which scientific knowledge is likely to be beneficial, be always filled up with a certain proportion of scientific men. Let discoveries be purchased by government, or let those who make them be otherwise rewarded, not according to the valuation of the discoverer or the caprice of official friends, but according to the judgment of able and competent committees. Where means are wanting to intellectual men let them be supplied. Such men may become the honour of their country, and they should therefore be early distinguished. Had some discerning spirit singled out Dalton in his first youth, and placed him in a position suited to his talents, who can tell what his mind might have achieved;—or had his researches been aided and facilitated in full manhood,—had he been snatched from the drudgery of a laborious occupation by the patronage of a liberal and enlightened people,—what farther discoveries might he not have effected?—Minds of a high and searching order should not be lightly squandered away, but should be religiously set apart to the prosecution of origi-

nal investigations in that tract to which nature most visibly inclines them. It will follow as one of the benefits of the national institution, that in its committees there will be men eminent and leading in every department, through whom the wishes of a great body of the lovers of science can be made known to government, and from whom again government can obtain information on all scientific subjects, and on the best and most advantageous method of promoting them.

After an able address by the Reverend Mr Harcourt, which, at the request of the meeting, is to be printed and circulated among the members of the association, it was moved and agreed to unanimously, that a society be formed to be called the British Association for the Advancement of Science. Other resolutions were then proposed, stating the objects of the society, and the qualifications for admission. The latter point gave rise to a long and protracted discussion, the result of which was, that the matter was referred to a committee, consisting of all persons present who had written and published on any scientific subject. It would have been much better management, and have saved much time, had the regulations to be proposed been previously made generally known, and submitted at once to a committee for revision. People came quite unprepared for the discussion of regulations they had not anticipated, and started objections, and proposed amendments on the spur of the moment, which, had they enjoyed the opportunity of a little forethought, they would either not have brought forward at all, or would have done so in a more regular and digested manner. Some little difficulties, however, were to be anticipated at the outset, and the entire arrangements in the end were so satisfactorily completed, that the small loss of time occasioned by the first day's discussion proved of very little importance.

At 5 P. M. a party of above 100 strangers and members of the Yorkshire Philosophical Society met at dinner in the York Tavern,—Lord Milton in the chair. The dinner was excellent, was enlivened by many speeches expressing zeal for science, and passed off in the most pleasant and cordial manner. At this and the succeeding public dinners, we had new proofs of

the attention and hospitality of the people of York, in the presents of fruit and venison, with which we were every day regaled.

At half-past eight in the evening the assembly again met in the Theatre and Museum. It had been arranged that Mr Abraham of Sheffield should on this evening deliver a lecture on magnetism, which he accordingly did to a very crowded audience. It contained as much important matter as would have made two excellent lectures in these degenerate days, was illustrated by many attractive experiments, and occupied the entire evening. Tea and coffee succeeded about eleven at night.

On future occasions it will be advisable, as is the case in Germany, that there should be neither lectures nor scientific papers read at the evening meetings. It is rather sleepy work in most cases to rise from the dinner-table, where men have been enjoying good cheer, and to sit down forthwith to listen patiently to a scientific lecturer. There are few persons whose vigilance will not at times be overcome by this test. All public scientific business should be dispatched before dinner, and the evenings should be reserved for conversation, for private and particular discussion, and for the cultivation of mutual acquaintance.

On Wednesday the 28th, the committee met at 10 A. M., to amend and complete the regulations of the association. At twelve they reported progress to the general public meeting, which assembled at that hour, after which the proper scientific business of the society was entered upon.

The first paper read was by Dr Brewster, on the crystallographic systems of Mohs, and on the propriety of adding to them a fifth, to be called the composite system, under which might be classed certain crystalline forms, not admissible into any of the four received systems. This paper, being of an abstruse nature, gave rise to no discussion.

The second paper was an able memoir on the philosophical character of Dr Priestley, by Dr Henry of Manchester. In this paper he endeavoured to account for and to justify the hasty and unfinished state in which Dr Priestley published his researches and discoveries. The science of chemistry was then

new,—other men were labouring in the field of discovery with equal ardour,—were occasionally stumbling upon the same facts, and laying claim each to what the other had already observed. Thus, to secure priority, it became absolutely necessary to be hasty in publishing. If this were true in the days of Priestley, how much more so must it be now?

A few remarks from Mr Luke Howard, on the character of Priestley's mind, concluded the business of this sitting.

Instead of dining at the ordinary to-day, the strangers formed themselves into little friendly dinner parties, in which men of like views and pursuits being associated together, could enjoy, along with the comforts of the table, the benefits of agreeable and quiet discussion. After the bustle and formality of the preceding day, an opportunity for more intimate communion was particularly desirable.

Our meeting in the evening was rather thinly attended. The ladies and all the lovers of sweet sounds had deserted us for Dr Camidge's concert, and none but those imbued with a true philosophic spirit found their way to the theatre. To this select audience, Mr Potter of Manchester exhibited his elegant microscope, an improvement on the reflecting microscope of Newton, and his elliptical mirrors, which as an amateur he has brought to great perfection.* The description and exhibition of Dr Brewster's Lithoscope, for distinguishing precious stones by the colours reflected from them under certain circumstances, followed by an explanation, with the aid of diagrams, of the principles on which it was founded, concluded the readings.

Thursday, Sept. 29th. At ten o'clock the committee again met, to prepare additional regulations, to suggest subjects of inquiry which it would be desirable to see taken up by scientific men, and to make various arrangements for the future management of the association.

At twelve o'clock it was announced to the general public meeting, that the association being now constituted, and having commenced its scientific career, it was necessary to its more perfect organization, that an annual president and certain other office-

* See p. 61 of this Number.

bearers should be chosen, and that provision should be made for holding the next annual meeting. It was then proposed at the recommendation of the committee, that Lord Milton be requested to take the chair as president of the association. This was carried by acclamation, after which his Lordship took the chair, and expressed his ardent desire to aid, by every means in his power, the objects of the association. It was then proposed that the Reverend Mr Vernon Harcourt should be named Vice-President, Mr Phillips secretary, and Mr Gray interim treasurer ; that Oxford should be the next place of meeting ; that Dr Buckland, who had expressed himself as cordially approving of the present association, should be President elect, and Dr Daubeny secretary ; and that the meeting at Oxford should take place in June, on a day to be named and announced by the committee at that place. To these it was afterwards added, that Dr Brewster and Professor Whewell of Cambridge should be Vice-Presidents elect, and the whole were unanimously agreed to.

Before entering upon the scientific business, his Lordship rose and expressed his regret, that he was under the necessity of leaving the meeting, and requested that Mr Harcourt might be permitted to take the chair.

The first paper read was by Mr Dalton, a very interesting and minute experimental inquiry into the relations that exist between the weight of food taken and that of the secretions and insensible perspiration. The experiments he had made upon himself about forty years ago, and, by estimating the two former, he endeavoured to draw conclusions as to the general amount of the latter. Mr Dalton must have early commenced his exact inquiries ; and it is exceedingly interesting to find him applying the discoveries and determinations of later science to numerical results obtained by himself nearly half a century ago, and eliciting by their aid new and important truths. The paper will appear in a volume of the *Manchester Transactions* now in the press.

The next paper was read by Mr Potter, in which he endeavoured to draw from the results of a set of ingenious experiments on the quantity of light reflected from certain simple

and compound metallic surfaces, what he considered to be a powerful objection to Fresnel's theory of light.

The third paper caused considerable discussion. It was upon the *whin-sill* of the northern counties, which the author, Mr William Hutton of Newcastle, had explored and examined throughout almost its whole extent. It is found not merely in dikes, but in the form of beds intervening between strata of almost every kind of rock occurring in the extensive district through which it passes. Such has been found to be the case also in other districts in regard to rocks of volcanic origin. Where the stratum is of small extent, as in the case of the *Arran pitch-stone* in the Island of Lamlash, it is easy to conceive that during a convulsion by which the rocks were upheaved, and the strata separated from each other, a quantity of melted matter from beneath might be injected into the lateral fissure, and on the return of tranquillity maintain its place there in the state of a bed entirely conformable with the other strata. But where the beds are of vast extent, stretching over a space of nearly a hundred miles, and are overlaid by thick and extensive deposits of other rocks,—it is difficult to conceive how an eruption could be powerful enough to cause fissures in the subjacent rocks, by which the fused mass should find a passage,—to upraise all the supercumbent strata, and to inject laterally a melted mass to a distance of many miles, and yet not be able to break through those upper, and probably less consolidated rocks also, so as to discharge the melted matter, as volcanoes now do, over the actual surface of the earth. This point occasioned considerable discussion. Professor Sedgwick considers the *whin-sill* to have been injected laterally over all the northern district, and therefore to be of a geognostic age later than that of the metalliferous limestone beneath which it lies. Mr Hutton took the opposite view, and exhibiting a section in which it occurred under beds of shale, limestone, and sandstone, all perfectly undisturbed and conformable. He argued, that, had the sill been injected laterally, a great mechanical force must have been exerted upon the upper rocks, of which no traces were to be found. He inferred, therefore, that the sill was older than these rocks; that it had been eject-

ed by volcanoes, then active, and pouring out their lavas over the whole surface of the earth ; and that the now overlying bed had been deposited on the surface of these lavas since the volcanoes became extinct.

Mr Murchison bore testimony to the ability of the paper, while he differed from the author in his conclusions. From the observations he had himself made upon the whin-sill, and from the apparent connection of the whin-dikes of Durham, the great Bolam dikes in particular, with the stratified basalt in question, he had no doubt that it had been injected laterally, and at a late date, not only into the carboniferous limestone series but into later rocks. At the same time the matter was still open to inquiry, and he considered it particularly desirable to trace whether the branches thrown off by the Bolam dike, and all bearing towards the whin-sill, had actually any connection with it.

Mr Phillips very plausibly argued that both parties might be in the right. He considered it probable that the sill had been ejected by an active volcano during the deposition of the metalliferous limestone, and was thus anterior to some beds, and posterior to others,—a position which he supported with his accustomed ingenuity.

The fourth paper was upon Vanadium, in which a short account of the discovery of the metal by Del Rio, by Sefström, and by myself, were given, and the mineral from which it is obtained, and several preparations and salts of the new metal were exhibited. The substance of this paper is inserted in the preceding number of this *Journal*. I was enabled to exhibit some exceedingly beautiful crystals of Vanadic acid formed by gradual cooling. They are in long flat prisms, reddish brown, with a shade of purple, transparent, of a high degree of lustre, possessing a refractive power nearly equal to that of the diamond, have regular double refraction, and belong to the prismatic system of Mohs.

The next paper was an able and interesting sketch of the ancient Flora by Mr Witham of Lartington. The results of his researches into the structure of fossil vegetables, and the light they throw on the character of the vegetation of former ages, were clearly and eloquently stated ; and all remained satis-

fied that Mr Witham had done much, and might yet do greatly more, towards the elucidation of this obscure, but to the geologist highly important subject.

A short paper by Dr Henry of Manchester, on the effect of roasting on the copper ores of Anglesea, closed the proceedings of this sitting. The ore, which contains from 5 to 20 per cent. of copper, is roasted, and by that means converted into lumps, which on examination are found to contain from 30 to 50 per cent. of copper. These lumps are picked out, and the metal separated by smelting. A similar observation had been made some years ago by Professor Bredberg of the School of Mines of Fahlun, and was detailed by him at length in a paper in the *Swedish Transactions*. A short abstract of this observation was inserted in this *Journal*, vol. iii. p. 357, to which the attention of the meeting was directed. After some observations by Dr Daubeny and Mr Phillips, the meeting adjourned.

Thus ended the scientific readings and discussions of the second day. All persons were satisfied, most men were delighted with it, and Mr Phillips, with that aptness which distinguishes him, did not fail in the evening meeting to quote the proceedings of this day, as an admirable illustration of the benefits to be derived from the association.

The dinner party to-day at the ordinary was an exceedingly pleasant one. There were present forty or fifty persons, among whom were Lord Morpeth and Sir George Cayley, Sir Thomas Brisbane, Mr Dalton, Dr Daubeny, &c. Mr Vernon Harcourt in the chair. The room was comfortably large for this number, and all the arrangements,—thanks here also to the York committee were excellent.

The assembly in the evening was unusually splendid. The ladies of York seemed anxious to make amends for their neglect of the previous evening, by extending to this night's meeting a double portion of their patronage. The Archbishop also, considering probably that science may be rendered a valuable hand-maid to religion, and that it is never more legitimately employed than when so serving her, attended with his family, and inserted his name in the subscription-book as a member of the association.

The lecture was delivered by the Reverend Mr Scoresby

of Liverpool, and consisted of "an exposition of some of the laws and phenomena of magnetic induction; with an account of a method of applying the magnetic influence to the determination of the thickness of rocks and other solid substances in situations where they are not otherwise measurable." This was an able and very elaborate paper. It is impossible from memory to give any proper outline of such a memoir, or to do justice to its talented author, and this is the less necessary, that it is likely to appear in an early part of the *Philosophical Transactions*.

After the lecture came as usual, tea, coffee, and conversation, and the meeting broke up at a late hour.

On Friday morning (30th) the committee again met at 10 A. M., and proceeded with the business arrangements of the society. At noon the general meeting took place, Mr Harcourt in the chair. The first paper was a continuation of Mr Scoresby's researches, containing the practical results, as to the measuring of rocks, walls, and similar objects, whose thickness it is desirable to know, but impossible to ascertain by ordinary means. This he determines by the deviation of the needle produced by his powerful magnets, which he finds to cause a perceptible and measurable angular deviation at a distance of sixty feet, and through the most solid bodies.

The second paper was by Dr Brewster, on the structure of the crystalline lens in the eyes of fishes. The curious modes of structure developed by Dr Brewster, as existing in the different tribes of fishes, add another to the numberless instances of wise adaptation of means to ends, which the works of nature display in their minutest parts.

Mr Murchison next detailed the very interesting observations of Mr Gilbertson, in regard to the occurrence in large quantities of the shells of existing species in the gravel and salt beds about Preston in Lancashire. Many of the shells were exhibited and recognized as species at present occurring on the coasts of Yorkshire and Lancashire. They are found as far as twenty miles inland, and at a height, if I recollect right, of at least an hundred feet above the present level of the sea. This curious fact, as it deserved, gave rise to considerable discussion, supported with much animation by Mr Murchison, Mr Phillips,

and Mr Greenough. There seems to have been some great general lifting up of the land in that part of the island, and it must have been so recent, that, had Britain been civilized in the days of the early Greeks or Romans, we should almost have expected to find some account of it in existing records.

Dr Daubeny of Oxford followed, with some interesting observations on the phenomena of hot springs. The points chiefly insisted on were, 1. The connection of hot springs with volcanic action, which led him into a short view of the theory of volcanoes; and 2. The occurrence of azote in such springs, and the mode of collecting and detecting it. This he illustrated by one or two simple experiments, showing with what facility the nature of the gases given off by a mineral spring might be made out; and he showed also how, by evaporating a quantity of the water to dryness, the solid contents might be obtained in a dry state, and preserved for analysis at a convenient opportunity.

This paper also gave rise to considerable and animated discussion, in which the leading geologists and some of the chemical men took a share. On the theory of volcanoes, it was observed, that, however simple and philosophical it might be to refer all to one cause, as Dr Daubeny and others do, which, by the aid of certain suppositions may be made to explain all the known phenomena, yet, as similar phenomena on a small scale are produced in our laboratories from many different causes, it may be so also in the great laboratory of nature, in which all recompositions and decompositions are continually going on on the greatest scale.

That azote in the gaseous state occurs often in mineral springs is well known; and Dr Daubeny has added several new examples of it in England; but that it occurs in a combined state has not been so generally made out. In the state of nitric acid it has occasionally been found in combination with potash, lime, and magnesia; and probably, if carefully sought for, might be more frequently met with. But azote has been found, and I am persuaded, if properly pursued, might be very frequently detected in springs in the state of ammonia. Berzelius obtained a large quantity of ammonia from the water of a spring at Ronneby, in the south of Sweden. It is desirable to search for it in all waters, especially such as give off free azote; but

if it do exist in them, it is evident, that, by evaporation to dryness, there is great risk of its being driven off. A very easy mode of detecting its presence would be to acidulate the water after concentration with sulphuric acid, by which the ammonia would be converted into sulphate, and rendered less volatile. Evaporate to dryness by a gentle heat, and add caustic potash, when ammonia, if present, will be evolved and rendered perceptible to the smell.

Among the friends and patrons of the society at York who paid kind and hospitable attention to those whom the love of science had brought to the meeting, the clergy must not be passed over in silence. They had been the zealous promoters of the meeting; had done much towards facilitating the preliminary arrangements; and exerted themselves by their influence and example to secure to the association that respect and general attention which it deserved, and which at York it amply received. To the church, therefore, the British Association is deeply indebted; and convinced, as I am, that true religion and true science ever lead to the same great end, manifesting and exalting the glory and goodness of the great object of our common worship, I trust that the firmer the association is established, and the more influential it becomes, the more willing and the more efficient an ally it will prove in the cause of religion. While in former times science was said to lead to infidelity, because then it was less profoundly studied, or with less zeal for truth, it is one of the happy characters of the science of this day, that it renders men more devout; and it is a pleasing evidence that such is the received opinion, when discerning and educated men—the friends and teachers of religion—of all ranks, step forward not only to patronize science, but to enlist themselves among its cultivators, and to distinguish those who have most successfully advanced it.

I have already adverted to the attendance of the Archbishop at the evening meetings. This day he gave a mark of his cordial approval and good wishes, by sending to the meeting a general invitation to dine at Bishopthorpe. Many of the strangers availed themselves of this opportunity to visit the ancient archiepiscopal palace so kindly thrown open to them; and all returned to the museum in the evening, gratified by the ho-

nour done them personally, and by the conviction that the association was proceeding under the most favourable auspices.

The first short paper this evening was by Mr Potter, on the analogy of the light exhibited by the electric spark in its passage through the Torricellian vacuum to that of the aurora borealis. This light he intended to exhibit, but unfortunately the apparatus had broken on its way to York.

Dr Warwick next exhibited Professor Moll's interesting experiment of forming a large temporary magnet of soft iron by the action of magnetism. Owing to an accidental defect, it did not succeed to the Doctor's satisfaction; but the horse-shoe was rendered sufficiently powerful to sustain nearly a hundred pounds

Dr Daubeney exhibited a little sphere of wire-gauze which, when immersed in water, filled, and when lifted out still retained the water. When shaken the water flowed out immediately from the pores. He explained the phenomenon on the principle of capillary attraction.

Mr Phillips closed the readings of the evening with a popular account of the new volcanic island in the Mediterranean by Mr Osborne. It was translated from the Malta Gazette, and some of the *sublime* parts caused considerable merriment.

Saturday the 31st, was the last of the public days. At ten o'clock the committee resumed their labours, and at twelve the public meeting took place.

The first paper was by Mr Dalton "on the specific gravity of the human body,"—an able and interesting memoir, which will appear in the *Manchester Transactions*. One of the chief points of inquiry was in regard to the mode by which we are enabled to support the great weight of the atmosphere pressing upon the human body, calculated to amount to from fifteen to twenty tons. Mr Dalton supposes all the pores of the body to be filled with air, and that this, with the air in the lungs, actually sustains all the pressure, leaving the solid parts unimpeded to perform their functions. The body in this case is just like an open vessel which, placed in an atmosphere of great density, remains uninjured, but which, if closed, and submitted to a similar pressure, would speedily be crushed together. From known experiments on the density of the human body, from

which it appeared, as we should naturally expect, that some men were lighter and some heavier than water, he took occasion to remark upon the absurdity of the common notion that all men could swim if their fears would permit them. "As well," said Mr Dalton, "might a piece of fir reproach a piece of lignum vitæ for sinking, as a light man reproach a heavy one for not being able to swim." And yet the common notion is not so absurd after all, if it be considered that fear makes a man empty his lungs of a great portion of the air which, were he courageous enough, would keep him afloat.

A long conversation followed the reading of this paper, in the course of which Mr Scoresby communicated some very interesting facts regarding the effects which the pressure of the water produced upon the whale at the great depths to which it often descended when struck by the harpoon. It often went down perpendicularly to the depth of a mile, but always came up exhausted and blowing out blood, showing that the pressure had so acted upon the vessels as to cause them to discharge a portion of their contents into the lungs.

Mr Allan, of Edinburgh, described a large specimen of aqua marine brought to this country by the Ex-Emperor of Brazil; after which, Mr Robison, Secretary to the Royal Society of Edinburgh, explained, by the aid of drawings, some contrivances he has introduced in the construction of his splendid lint-seed oil barometer, to enable him to free its contents from elastic fluids when first filled, and to preserve them from contamination by subsequent absorption from the atmosphere.

Dr Brewster exhibited his alum and rock-salt prisms, so far surpassing, and at so trifling an expence, the finest glass prisms made in England; showing the lines in the spectrum more clearly even than the homogeneous prisms of Fraunhofer's own manufacture. To find a cheap and common material in the hands of a man of genius substituted with manifest advantage for a rare and expensive prism, is one of those happy economical adaptations of common means which we rarely meet with, but the importance of which, when we do meet with them, are comprehended by all.

This led also to an explanation of Dr Brewster's views regarding the heating rays supposed by Herschel to be

most numerous in the dark part of the spectrum, as there he found the temperature highest. This opinion was shown by Seebeck to require considerable modification. By using hollow prisms filled with fluids, he found the maximum of temperature to have a different locality for each substance, so that when a solution of sal-ammoniac was used the greatest heat was in the yellow rays. It is not always, therefore, as Herschel found it, beyond the red rays. But by darkening his prism, Fraunhofer lengthened his spectrum very considerably, so as to prove that there was light for a certain space beyond the limits of the spectrum, as known to Herschel; and by eating out, as Dr Brewster calls it, that is, by using prisms which absorb the most intense light, he has succeeded in further extending even Fraunhofer's spectrum, and showing that light is diffused over all the space in which Herschel recognized his heating rays,—and thus has proved that there are no rays of heat unaccompanied by light, and therefore none which we can pronounce to be heating rays alone.

The subject of colouring matters also was touched upon. Chemists used formerly to talk of colouring matters, and to say that such a substance was coloured by such another. But a coloured body by union with a colourless substance sometimes gives a colourless compound, and often one of a different colour from that of the substance in its separate state. The colour, therefore, must be due to a corpuscular arrangement; and where we find that a coloured body by union with a colourless body forms a coloured compound, we can legitimately say or infer, not that the one body has imparted its colour to the other, but that the union of the two has taken place in such a way as to cause an arrangement of particles capable of producing the same effect upon light. And further, as change of colour may take place, as it often does, by simple heating, without affecting the optical properties of the substance, we cannot say that colour is owing to *optical structure*, but to a change in the ultimate atomic arrangement only, while we are enabled to infer further that different modes of corpuscular arrangement are consistent with and may produce the same optical structure.

On the close of this discussion, Mr Forbes read his able and

elaborate paper on the horary oscillations of the barometer, which was listened to with great attention; but the nature of the subject prevented any observations from being made upon it.

A letter from Sir James South to Dr Brewster was read, in which he directed the attention of the meeting especially to the satellites of Jupiter. They were generally supposed to disappear when within the disc of the planet, but he had lately observed one of them like a black spot on its surface,—and he wished to know why they were not always so visible.

The business being now closed, the meeting broke up.

The evening assembly was splendid as usual, the rank and fashion of York giving us their countenance to the last. Dr Daubeny opened the meeting by explaining some experiments of the Reverend Mr Taylor, of York, for the purpose of increasing the light without increasing the consumption of gas. These were followed by some ingenious applications of the heat of gas to economical purposes by Mr Robison of Edinburgh, illustrated by a few simple experiments. The Reverend Mr Harcourt then exhibited and explained the principle of a new and simple lamp for giving a good light, at a cheap rate, by the consumption of the inferior qualities of oil. Dr Brewster's admirable memoir on a new analysis of solar light was next read by Mr Phillips, and illustrated by diagrams; at the close of which some discussion took place, chiefly concerning the peculiarity of vision to which Mr Dalton is subject.

A translation of a memoir by Professor Gazzeri of Florence, "on a method of rendering visible the traces of erased writing," was then read by Mr Gray. The method consisted chiefly in the application of heat to the paper from which the erasure had been made. It called forth some observations from Drs Daubeny and Brewster, during which the latter pointed out the utility of heat in enabling antiquarians to make out the legend on old coins; and stated, that he had never been more struck than by observing once on an old coin which he had placed on hot iron an inscription, previously invisible, make its appearance, which he easily read in the dark, and found to be "*Benedictum sit nomen Dei.*"

This finished the business of the evening and of the first

meeting of the British Association. Lord Morpeth then rose, and addressed the meeting as follows :—

“ Ladies and Gentlemen, an office has been assigned to me, which, although most entirely without any qualification or pretension to fulfil, I nevertheless accept, and will discharge, to the utmost of my ability, with the utmost alacrity. To the character of a man of science I have, unfortunately for myself, no claim whatsoever ; but I have the good fortune to be intimately connected with the county, and consequently with the city of York ; and I feel that they have both received great benefit and additional credit from the meeting which is now brought to a conclusion. I say this both with reference to the positive instruction we have received upon so many most interesting and important subjects, and also to the circumstance of this town and this edifice, already so much indebted to the zeal, perseverance, and ability of our Vice-President, having been now selected as the birth place of an association, which, I trust, is destined to confer fresh lustre on British science, (applause,) to give a new motive and a new guarantee to the friendly intercourse and continued concord of nations ; to make farther inroads into the untravelled realm of discovery, and glean fresh harvests from the unexhausted field of Nature ; to promote the comforts and augment the resources of civilized man ; and to exalt above and over all the wonder-working hand of Heaven. For it will always come out as surely as from the rusty medal of which we have this moment heard, ‘ *Benedictum sit nomen Dei.*’ Observe well, if you wish to appreciate rightly the true value and nobility of science, that while it proposes to itself distinct courses and definite spheres of its own, its general tendencies conduce to peace, and minister to piety. With these views and these hopes, it is natural and it is becoming that there should be mixed feelings of gratitude to those whose efforts have contributed so largely to our future progress. An assembly like that which I have the honour to address, will appreciate far more justly than I can pretend to do, the several papers and productions which have been submitted to our notice ; I have no scruple in leaving to your more competent and accurate discrimination, the indications of enlightened and powerful thought which they have exhibited ; but I feel sure

that, if you pardon me for this intrusion of myself, the proposition I now make will command, upon this occasion, both the grave assent of science and the soft sanction of beauty. I move that the thanks of this meeting be given to Dr Brewster, and the other authors who have favoured us with their communications.*

This motion was carried by acclamation, after which Mr Murchison rose, and, "on the part of Dr Brewster and his other scientific friends, begged leave to return thanks for the high honour done to the contributors of scientific memoirs, and for the kind assistance and valuable aid which had been received from the residents of York and its neighbourhood, in the promotion of the objects of this meeting. He explained the motives which first induced the original promoters of the meeting to select the city of York for their first assembly. To this city, as the cradle of the association, they should ever look back with gratitude; and whether they met hereafter on the banks of the Isis, the Cam, or the Forth; to this spot, to this beautiful building, they would still fondly revert, and hail with delight the period at which in their gyration they should return to this the point of their first attraction. Mr Murchison, after warmly eulogizing the kind reception and hospitality which the strangers had experienced from the Archbishop, and from all classes of the inhabitants of the city and neighbourhood, concluded, amidst loud applause, with a motion of thanks as follows:—"That the cultivators of science here assembled, do return their most grateful thanks to His Grace the Archbishop of York, the Patron, and to the Officers and Members of the Yorkshire Philosophical Society, for the very liberal manner in which, by the use of their Halls and Museum, and by their obliging and unwearied efforts to provide every accommodation and comfort to the visitors, they so essentially contributed to the success and prosperity of this Association."

This motion was seconded by Dr Brewster, and warmly supported by Mr Dalton. Mr Vernon Harcourt, who was in the chair, then said, that "it was quite unnecessary, from the feelings which he knew to pervade the breasts of all, both scienti-

* *Yorkshire Gazette*, October 8th 1831.

fic strangers and residents, to put to the vote of the meeting either of the proposals so eloquently brought forward. In the long period of its existence, the ancient city of York had never greater reason to be proud than of the genius and talent it contained within its walls at that moment, and of the honour it had obtained of being the birth-place of an Association destined (he firmly believed) greatly to enlarge the boundaries of science." After some farther observations, he declared the meeting to be adjourned to Oxford.

This evening was a pleasant one to all parties, and all, I believe, felt regret that the sittings had come to a close.

On Monday the committee held its last meeting, and embodied the latest suggestions into the regulations of the Society.

The following copy of the Circular, drawn up by the Vice-President and Secretary, contains these regulations in their finished state, and presents a view of the nature of the association.

OBJECTS.

The Association contemplates no interference with the ground occupied by other Institutions. Its objects are,—To give a stronger impulse and a more systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate science in different parts of the British Empire, with one another, and with foreign philosophers,—to obtain a more general attention to the objects of science, and a removal of any disadvantages of a public kind which impede its progress.

RULES.

Members.—All persons who have attended the first meeting shall be entitled to become members of the Association, upon subscribing an obligation to conform to its rules.

The fellows and members of chartered societies in the British Empire shall be entitled, in like manner, to become members of the Association.

The office-bearers and members of the councils, or managing committees, of philosophical institutions shall be entitled, in like manner, to become members of the Association.

All members of a philosophical institution recommended by its council or managing committee, shall be entitled, in like manner, to become members of the Association.

Persons not belonging to such institutions, shall be eligible, upon recommendation of the general committee, to become members of the Association.

Subscriptions.—The amount of the annual subscription shall be One pound, to be paid in advance upon admission; and the amount of the composition in lieu thereof, Five pounds.

Subscriptions shall be received by the Treasurer or Secretaries.

Meetings.—The Association shall meet annually, for one week, or longer. The place of each meeting shall be appointed by the general committee at the previous meeting; and the arrangements for it shall be entrusted to the officers of the Association.

General Committee.—The general committee shall sit during the time of the meeting, or longer, to transact the business of the Association. It shall consist of all members present, who have communicated any scientific paper to a philosophical society, which paper has been printed in its transactions or with its concurrence.

Members of philosophical institutions, being members of this Association, who may be sent as deputies to any meeting of the Association, shall be members of the committee for that meeting.

Sub-Committees.—The general committee shall appoint, at each meeting, sub-committees, consisting severally of the members most conversant with the several branches of science, to advise together for the advancement thereof.

The sub-committees shall report what subjects of investigation they would particularly recommend to be prosecuted during the ensuing year, and brought under consideration at the next meeting. They shall engage their own members, or others, to undertake such investigations; and where the object admits of being assisted by the exertions of scientific bodies, they shall state the particulars in which it might be desirable for the general committee to solicit the co-operation of such bodies.

The sub-committees shall procure reports on the state and progress of particular sciences, to be drawn up from time to time by competent persons, for the information of the annual meetings.

Local Committees.—Local committees shall be appointed, where necessary, by the general committee, or by the officers of the Association, to assist in promoting its objects.

Committees shall have the power of adding to their numbers those members of the Association whose assistance they may desire.

Officers.—A president, two vice-presidents, two or more secretaries, and a treasurer, shall be annually appointed by the general committee.

Papers and Communications.—The general committee shall appoint at each meeting a sub-committee, to examine the papers which have been read, and the register of communications; to report what ought to be published, and to recommend the manner of publication. The author of any paper or communication shall be at liberty to reserve his right of property therein.

Accounts.—The accounts of the Association shall be audited annually by auditors appointed by the meeting.

OFFICERS OF THE ASSOCIATION.

President.—Charles William, Viscount Milton, F. R. S. &c. President of the Yorkshire Philosophical Society.

President elect.—Rev. William Buckland, D. D. F. R. S. &c. Professor of Geology and Mineralogy, Oxford.

Vice-President.—Rev. William Vernon Harcourt, F. R. S. &c. Vice-President of the Yorkshire Philosophical Society.

Vice-Presidents elect.—David Brewster, LL. D. F. R. S. L. & E. Corresp. Member of the Institute of France. Rev. William Whewell, F. R. S. &c. Professor of Mineralogy, Cambridge.

Treasurer.—Jona. Gray, Esq. York.

Secretaries.—*York.*—William Gray, Jun. John Phillips, F. G. S. &c. Secretaries of the Yorkshire Philosophical Society.

London.—Rev. J. Yates, F. L. S., G. S. &c.

Edinburgh.—J. Robison, Sec. R. S. E. &c.

Dublin.—.....

Oxford.—Charles Daubeny, M. D. F. R. S. Professor of Chemistry, Oxford. Rev. Baden Powell, A. M., Savilian Professor of Mathematics, Oxford.

LOCAL COMMITTEES.

London.—G. B. Greenough, F. R. S. Vice-President of the Geological Society. R. I. Murchison, F. R. S. President of the Geological Society. Rev. James Yates, F. L. S. &c.

Edinburgh.—James D. Forbes, F. R. S. E. J. F. W. Johnston, A. M. John Robison, Sec. R. S. E. &c.

Dublin.—W. R. Hamilton, F. R. S. &c. Astronomer Royal of Ireland. Rev. B. Lloyd, D. D. Provost of Trinity College, Dublin.

India.—George Swinton, Esq. Chief Secretary to the Government in India, has been requested to form a Committee at Calcutta, with the aid of Major Benson, J. Calder, Esq. Dr Christie, J. Herbert, Esq. J. A. Prinsep, Esq. and Sir Edward Ryan.

SUB-COMMITTEES.

Mathematical and Physical Science—David Brewster, LL. D. F. R. S. L. & E. &c. Sir Thomas Brisbane, K. C. B. F. R. S. L. & E., Corresp. Member of the Institute of France. James D. Forbes, F. R. S. E. W. R. Hamilton, F. R. S. &c. Rev. William Pearson, LL. D. F. R. S. Vice-President of the Astronomical Society. Rev. William Scoresby, F. R. S. L. & E. Corresp. Member of the Institute of France. Rev. W. Whewell, F. R. S. &c.

Chemistry.—Rev. John Cumming, F. R. S. Professor of Chemistry, Cambridge. John Dalton, F. R. S., President of the Literary and Philosophical Society at Manchester, Corresp. Member of the Institute of France. Charles Daubeny, M. D. F. R. S. &c. Rev. W. V. Harcourt, F. R. S., &c. J. F. W. Johnston, A. M. William West, Secretary of the Leeds Philosophical Society.

Mineralogy.—Thomas Allan, F. R. S. L. & E. Robert Allan, F. G. S. &c. David Brewster, LL. D., F. R. S. &c. J. F. W. Johnston, A. M. Rev. W. Whewell, F. R. S. &c.

Geology and Geography.—Rev. William Buckland, D. D. F. R. S. &c. Rev. W. D. Conybeare, F. R. S. &c. Vice-President of the Geological Society, Corresp. Member of the Institute of France. Sir Philip Grey Egerton, Bart. F. R. S., G. S. &c. James D. Forbes, F. R. S. E. F. G. S., G. B. Greenough, F. R. S. &c. William Hutton, F. G. S. &c. R. I. Murchison, F. R. S. &c. John Phillips, F. G. S. &c. Rev. Adam Sedgwick, F. R. S., G. S. &c. Woodwardian Professor, Cambridge. William Smith, Author of the Geological Map of England. Henry Witham, F. G. S. &c. Rev. James Yates, F. L. S., G. S. &c.

Zoology and Botany.—Charles Daubeny, M. D. F. R. S. &c. Rev. J. S. Henslow, F. L. S. G. S. Professor of Botany, Cambridge. J. C. Prichard, M. D. F. R. S.

Mechanical Arts.—J. H. Abraham, F. L. S. &c. John Robison, Sec. R. S. E. &c. Benjamin Rotch, F. S. A. &c.

At the request of the Association, Professor Airy has undertaken to prepare for the next meeting a report of the state and progress of Astronomy ; Dr Brewster a similar report on Optics ; Professor Whewell, on Mineralogy ; J. F. W. Johnston, Esq. on Chemistry ; and J. D. Forbes, Esq. on Meteorology.

By direction of the General Committee, a full report of the proceedings of the General Meeting at York, including a statement of the scientific subjects proposed for inquiry, will be prepared for

32 Mr Johnston's *Account of the Scientific Meeting, &c.*

publication by the officers of the Association, and distributed *gratis* to every member.

The next Meeting of the Association is appointed to be held at Oxford, in the month of June 1832.

Such is an outline of the formation, nature, and first proceedings of the British Association for the promotion of science. The time is favourable for the formation of such a national institution. For if science in England be on the decline, as some maintain, what more likely to call forth new efforts, by which it may be restored, than the youthful exertions of a great scientific body? If it be not on the decline, then the founders of the British Association have only adopted the wisest and most likely means to maintain and increase the reputation of the country; and, by bringing forward the genius and zeal of those devoted to philosophy, to silence the outcry, and disprove the assertions of those who maintain science to be rapidly on the wane. In either view, then, the establishment of the society must be an unmixed good; and it ought, therefore, to meet with the cordial support both of those who assert, and of those who deny, the existence of a decline. Here is a neutral ground on which both parties may meet and conjoin their most strenuous exertions,—the one to maintain the present, and the other to restore the past, scientific eminence of British philosophers. The association having but one grand object in view, the promotion of science through the combined exertions of men devoted to different branches of the same great study,—will pursue that end by the employment of those means which the general opinion of the most competent judges shall decide upon as the most likely to effect the greatest and most permanent good. Minds are already actively engaged and hands set busily to work; and when a few years shall have given time and opportunity for employing and giving efficacy to the various methods of improving science which it has in view, we may hope to find each succeeding anniversary more fruitful in new and important results of scientific labour.

PORTOBELLO,

25th November 1831.

ART. II.—*Observations on the Decline of Science in England, and the Means of its Revival.* Extracted from "*The Prospects of Britain.*" By JAMES DOUGLAS, Esq. of Cavers. Edinburgh, 1831.

"England cannot afford to be in arrear of any other nation in the progress of useful improvement."—*Huskisson's Speech on the Shipping Interest.*

THERE are few of our readers, we believe, who have any knowledge of the state of science in England, and whose judgments are not misled by personal or national vanity, who do not believe that her sciences and scientific arts have been long on the decline. This, as we have already shown, is the expressed opinion of the great men who now stand at the head of English science. It is the opinion also of philosophers resident in distant lands, who are deeply attached to their country, and deeply interested in its scientific renown; and we shall now see that it is the opinion also of men distinguished by piety and eloquence, whose opinions are placed above all suspicion of self-interest, and who bewail the decline of science, and the growing indifference to the intellectual honour of the nation as the harbinger of national degradation and ruin.

The newly published work in which these views are contained is a pamphlet entitled "*The Prospects of Britain,*" by James Douglas, Esq. of Cavers, a gentleman already well known to the Christian world, by various works distinguished by piety, learning, and eloquence. The pamphlet to which we now refer, contains a rapid sketch of the religious, the moral, the intellectual, and the political state of England. With the boldness of a Christian patriot, and with an enthusiasm which high principles alone can sustain, the author points out the corruptions which degrade our public institutions; he states and explains the means by which the national interests may be revived; and he unfolds the prospects which may still be cherished if these means shall be blessed with success. The following part of the pamphlet is that to which our attention is especially called.

"Though Britain is thus debarred from physical greatness, there yet lies within her reach a moral supremacy, which to

the latest ages might preserve her pre-eminence among mankind. Superiority in force and in numbers must be conceded to those rising empires, compared to whose territories these islands are but specks amid the waves. But superiority in knowledge and in art, in moral worth, and in the benefits conferred on other nations, might amply compensate for a limited territory. The widest extended empire has its bounds, and to pass these is no longer an addition of strength but of weakness. To the progression of knowledge there is no limit; for while generation succeeds to generation, each will add some new facts to science, and advance to a higher eminence than that which their predecessors occupied. The design of Providence is progression in the individual and in the species. Perfection is placed beyond our reach, but the path to approach it is ever open, and, as wave urges wave, so each following age presses forward on the preceding. Though all nations, even in spite of themselves, are borne forward by the current, yet the state which takes full advantage of this tide in human affairs, may still hope to keep ahead of the rest, and to preserve the pre-eminence which it once has attained.

“Pre-eminence in knowledge is necessary to the existence of Britain. In the resources of an extended territory, she is far behind the great nations of Europe,—France, Austria, and Russia,—not to mention the rising states of America; but, in the immediate application of moral energy and intelligence to all the purposes of a high civilization, no country can compete with Britain, were she enlightened as to her interests, and true to herself.

“There are two great instruments for the promotion of science, in addition to other means now employed, the formation of a general and voluntary society, and the enlarged patronage of the state. No other country, with a nearly equal portion of liberty, ever did so little for science as Britain, in proportion to its riches, population and extent. Whatever has been done has been effected by the efforts of individuals, not by the aid and encouragement of the state. The free cities of ancient and modern times have in general been eminent for their patronage of science and of art; but the English seem anxious to justify the accusations brought against them,—that they are

merely a nation of shopkeepers, and that their thoughts never soar above the profit and loss of their traffic.

“ Though the expenditure of two or three hundred thousand pounds a-year, wisely bestowed in the promotion of science and of art, would be true economy, and would multiply to a much greater extent the national resources, yet there are numbers in Britain so ignorant of their true interests, as to count the tenth part of that sum so bestowed a censurable waste of the public money. Millions after millions have been drawn from them to be squandered in wars entirely foreign to the welfare of Britain, while a comparatively small sum, laid out to the greatest advantage for the country, could scarcely be raised without many murmurs, and without the reprobation of many pretended patriots.

“ However no degree of ignorance is invincible by the continual repetition of truth. If the friends of knowledge, as they are now generally aware how rapidly science is declining in this country, would repeatedly bring the subject forward, whenever they have a fit opportunity, the duty and paramount importance of the state giving its vigorous aid to knowledge, would at length be admitted, and science would meet with that due encouragement, which in this country it has never yet received.

“ Yet in this country such encouragement is more needed than in any other. By the earnest pursuit of gain, and the fervour of political partizanship, men's minds are here more distracted from the higher walks of science, than any where else. In countries where there are few tempting outlets, in poor and remote districts which retain their primitive condition far from the bustle and active stir of commerce, literature is almost the only occupation that presents itself, and there are no temptations to draw the mind to other pursuits. In many parts of the interior of Germany, a student of abstract science is no poorer from devoting his time to occupations which afford so small a recompense. All are poor around him, and he has not the discouraging contrast to draw, which the Englishman must do, when he would chuse between the highest range of science, and the gainful occupations which everywhere present themselves to him.

“ But the scientific pursuits which are least remunerative to the student himself, contribute ultimately most to the advantage of the country. The higher regions of science, though barren to the cultivator, are the well-heads from which the lower departments are refreshed and watered. If science be cultivated, there is no danger in a commercial country that arts should be neglected ; but it is very possible that the arts themselves may be busily attended to, where the sciences, from whose root they spring, are secretly going to decay.

“ Without a fixed income, and an honourable place attached to them in society, it would be in vain to expect, that men of the highest genius should sacrifice themselves for a country so ignorant, as not even to be able to appreciate that sacrifice, and so blind, while it is wasting its treasures in projects foreign or hurtful to it, as not to bestow a pittance on the men whose talents would constitute its true glory and surest defence. But if God spare this country, we may hope and pray that He will give it a better mind, that it will become quicker to discern, and not slow to reward those geniuses, whom God has enabled to trace his operations, and who, when their study of nature is enlightened and guided by the Divine Spirit, are among the best gifts that the Father of Lights bestows upon the children of men.

“ But the highest encouragement which science could receive must result from the union of the patronage of Government, with the interest taken in its prosperity by the nation at large. This interest could only be universally diffused by a general and voluntary society. We have already pointed out the form of such a society in ‘ The Advancement of Society,’ and need not repeat here what has already been said. Without this general co-operation, whatever pensions Government might bestow, would soon cease to be distributed to merit, and, like all other contributions professing to be for the public service, but which the public itself does not watch over, would soon be converted into private and unmerited emolument. Neither, on the other hand, could a voluntary society, if left unaided by Government, afford certain and permanent remuneration, nor would its contributions be so honourably received if appropriated to individuals as salaries bestowed by the state, and conferred as

marks of public honour and approbation. But an institution, which would be an improvement and enlargement of the National Institute of France, and where the election of new members should have more of a national character, and if to each member a respectable salary was attached, such an institution would soon re-animate the decaying pursuit after scientific truth, and place this country at the head of all others, for the patronage and possession of knowledge, instead of miserably lagging in the rear of even second-rate states. While a general voluntary society, spreading its ramifications through every district of the country, however remote, corresponding with the national institution, seconding all its efforts, and diffusing with rapidity its information, stirring up the mind of the country, and suffering, throughout it, no degree of talent to slumber, bringing merit out of obscurity, and exciting genius to its full speed, would dispel the darkness from every corner of our land, and make it throughout one blaze of light.

“ It is impossible to estimate the quantity of talent that is latent in every country, and that passes unheeded and unexercised from the cradle to the grave. Indolence is most natural to man, and where there is no necessity there is no exertion. Some degree of thought is required for the ordinary conduct both of individuals and of states ; but where there is a political calm, and affairs go on in their usual train, the general mind is scarcely stirred, and it is only in political storms that we can form some notion of the variety of gifts and powers, which are widely distributed amongst men by the Author of their being.

“ These latent resources have never been exhausted or completely fathomed ; however great the emergency, the talents it calls out are always commensurate with the occasion, provided the state does not sink under the blow of calamity, but is rising to repel it. We should never have known what efforts human nature is capable of, nor the number of great minds which can be reared out of a scanty population, and that not for one age only, but unexhausted in number and variety during successive generations, had we not before our eyes the small and barren territory of Attica, with its scanty free population, yet so overfruitful in master minds, as to think itself obliged to expel them

by the ostracism, or to condemn them to death ; and, in the midst of all this thinning, never to fail in the supply, till its freedom failed ; and even in the wane of its age and liberty, still to be fertile in great thoughts, and to produce eminent philosophers, when it no longer afforded a field for its unrivalled statesmen. But Britain, could it rise to the same point of effort and energy, would be equal to hundreds of Athenes combined. There are slumbering within the seas that contain her, innumerable minds of the same temper with those which scattered the hosts of the Persians, or employed the divine language of Greece in conveying thoughts, which can never perish till the race of men are extinct. Britain also possesses the elements of a government, superior to what Solon could bestow upon Athens, resting on far deeper foundations, and capable of a higher elevation. We are freed from slavery, which was the canker of Greece and Rome. By means of the press and education, we have a larger national assembly to hang on the words of any Demosthenes who may arise amongst us, than all the free states of antiquity contained, when freedom expanded its broadest wing over the favoured shores of the Mediterranean ; and we have a nobility, if God would restore to them the heroic and devout mind of their ancestors at the Reformation and Revolution, who might vie, in the eminence of their position, with the senators of Rome or the nobility of Venice ; and a throne, which offers occasions of devotion to the welfare of the country, not inferior to those imagined by the tragic poets, to have been presented to the self-devoted chiefs of the heroic ages.”—Pp. 55—63.

Such are the sentiments and views of Mr Douglas. Their coincidence with those of Mr Babbage and his friends cannot fail to strike our readers ; and we would beg also to direct their attention to the important fact, that “ *the British Association for the Promotion of Science*,” which was established by the meeting of the Cultivators of Science lately held at York, seems to fulfil the great object which Mr Douglas had in view by proposing his “ *Advancement Society*.” It remains for the British government to carry into effect the other national measures which Mr Douglas has advocated, and which we have

always considered as essentially necessary to the revival of science, and to the promotion of the scientific arts.

Before concluding this notice, we would call the attention of our readers to the following article, written by a mathematician and a natural philosopher of the first eminence, whose services to one of the most useful branches of practical science have extended his reputation throughout Europe.

ART. III.—*On the direct Encouragement afforded to Science by the French Government.* Communicated by a Correspondent.

A REPORT on the Public Accounts of France has lately been printed by order of the House of Commons, and there are contained in it facts bearing on the momentous question of the decline of science, which come home to the bosom of the English philosopher with tremendous force, arousing all his indignant feelings, and calling into activity the generous sympathies of a heart at all times feelingly alive to the higher interests of man.

We purpose in this very brief paper to call the attention of our readers to two particulars connected with the report in question: *first*, that the claims of science are **DISTINCTLY RECOGNIZED** by the government of France; that public offices exist for the management of its important details; and that a distinct section of what is there termed “the Nomenclature of the Expences of the Ministry of the Interior,” is exclusively devoted to it;—and *secondly*, that this patronage is substantial, and consists of something more than barren compliment and praise; that very large sums are annually expended in the support of scientific and literary establishments, and for the noble purposes of direct “encouragement to learned men.”

We shall find, moreover, in tracing the history of times that are past, that this same system of encouragement has not been confined to one particular mode of government in that country, but formed a distinguishing feature of the ancient regime, of the revolution, of the imperial reign of Napoleon (Note A,) of the time of the restoration, as well as of the period of the “movement” under King Philippe;—that it has long formed and now forms an integral part of the French government and

people; that it is part and parcel of their public institutions; and that science, *transcendental science*, has found in the very heart of the governing power of the state, "a local habitation and a name." The present condition of the exact sciences in France, forming as they do the base and "Corinthian capital" of physical knowledge, affords a brilliant and decisive proof of the benefits which the fostering power of the state is able to bestow. The *Ecole Normale* alone carried them up to a pitch of unrivalled splendour, and the public taste received a wholesome direction from it. The "past and present state of the system of the world," to borrow an expression of Playfair, "was exhibited in formulæ of transcendent beauty, by *men whose energies had been awakened and sustained by the supreme power of the state.*" The world never before received an impulse like it, and for a time the human mind seemed to bend beneath the enormous load. It was then that the *Mécanique Celeste* became the touchstone of human ability, and the standard by which human attainments were to be measured. The greater part of men crouched in humility and fear before it; and only here and there, in the deep silence which its appearance inspired, was an individual to be found capable of grappling with its lofty and magnificent contents. "He reads the celestial mechanics of Laplace," was the proud remark of a father, eager to impress a distinguished statesman with a favourable opinion of his son, at a time when triple integrals and the calculus of variations were but little known in England; but it must have been a death blow to the ardent hopes of the parent, when he heard the cold and discouraging remark, that far humbler attainments were best suited to the condition in which his son was likely to move.

The Ledger of the Ministry of the Interior is divided into nine sections, each of which has its own subdivisions. The particulars of the fourth section are below :

Nomenclature of the Expences of the Ministry of the Interior.

Designation of the Services.	Division and Offices of the Ministry charged with the Administration of the different Services.	
	Divisions.	Offices.

SECTION IV.

Sciences, Literature, and the
Fine Arts.

Chap. XVIII.—Scientific or Literary Establishments.	Sciences, Literature, & the Fine Arts.	Office of Sciences.
Chap. XIX.—Establishments for Fine Arts, Monuments of Bronze or Marble.	Ditto.	Office of Fine Arts.
Chap. XX.—Encouragement and subscriptions to Artists and Learned Men.	Ditto.	Ditto of Office of Sciences.

Here then we have offices for the sciences and the fine arts, and for the encouragement of learned men actually existing, and the expences of which for the year 1831 were ordered by the Minister, the 14th of November 1830. What the actual amount ordered was, the Report does not state, in consequence of its having been drawn up before the expiration of the year ; but every particular for the year 1828 is given as follows :

Detail, by Chapters, of the definitive Account of the Expences of the Ministry of the Interior for the year 1828.—SECTION IV.

Chapter of the Expences.	Credits Granted.	Expences resulting from services performed.	Claims proved for Creditors of the State.
Scientific and Literary Establishments, -	1,573,000 00	1,572,987	25
Establishments for the Fine Arts, Monuments in Bronze or in Marble,	453,000 00	431,423	51
Encouragements and subscriptions in favour of Artists and Literary Men,	382,000 00	413,978	98

Here then we find more than a million and a half of francs actually *paid* towards the support of scientific and literary establishments ; nearly half a million for the fine arts, and more than four hundred thousand francs as direct encouragements and subscriptions for learned men. What a contrast to England, which has lately seen the ten small pensions awarded by

George IV. to some of the most learned and excellent men of the day suddenly stopped!!

In the accounts of the ministry of commerce and manufactures, we also find the following particulars relative to the conservatory and school of arts and trades.

Detail, by Chapters, of the Definitive Account of Expences incurred by the late Ministry of Commerce and Manufactures in 1828.—SECTION IV.

Chapters of the Expences.	Credits granted by the Finance Laws.	Payments made on Orders.
Conservatory and School of Arts and Trades, -	392,661 00	390,506 28

A liberal and Methodical system like this cannot but be productive of great advantage; and no wonder that the mathematical and physical sciences have so abundantly flourished in a country where the state has so pre-eminently fostered them. We hope we shall ere long see a column of the great Ledger of the Treasury of England opened for this splendid and magnificent purpose, to revive the drooping energies of English science;—that her great men,—for she has still great men,—who now pine in obscurity, and who have so long hanged their harps upon the willows, may, in the great system of changes now in active preparation around us, reassert their undoubted intellectual superiority, and that Britain, as she has long been the first in arms, may also be the first in every thing that imparts dignity to the mind of man.

Note A.—On the occasion of presenting the celebrated Report to Napoleon on the progress of the Sciences from 1789 to 1808, the Institute had it in command, not only to report on the actual state of knowledge, but to suggest the measures that would promote its further advancement. The cultivation of transcendental analysis, and giving to France “une perpendiculaire digne de sa meridienne,” were the noble and disinterested requests of Delambre, the first of the perpetual secretaries. To maintain the emulation *already existing* among the cultivators of science, and *to secure to them successors of equal ability and zeal*, were the ardent desires of Cuvier. “Of all the establishments formed, and of all the labours undertaken by the

command of Alexander," said Cuvier, "Aristotle's *History of Animals* is the only one which now remains an everlasting testimony of the love of that great prince for natural knowledge. A word from your Majesty can create a work which shall as much surpass that of Aristotle in extent, as your actions surpass in splendour those of the Macedonian Conqueror."—"I was willing," replied Napoleon, "to be informed of what remained for me to do to encourage your labours, that I might console myself for not being able otherwise to contribute to their success."

What was said by Delambre and Cuvier represented the feelings of the whole French Institute. The language has been charged by some with adulation; but it had a far higher and nobler principle for its source. It was felt that the influence of him in whom the supreme power of the state was concentrated, could accelerate the march of the sciences, and the sentiments therefore were those of dutiful respect. What an impulse would be communicated to science in the three capitals,—in every subordinate city,—in the remotest corner of the empire, were King William at this moment to call around him the most distinguished cultivators of science in the British islands, and demand what he could do for the advancement of natural knowledge.

ART. IV.—*An Account of some Experiments on Olefiant Gas.*

By JOHN DAVY, M. D. F. R. S. Assistant Inspector of Army Hospitals.

MY DEAR SIR,

MALTA, May 26, 1831.

IN looking over some old papers, which I have lately received from England, I found one written in the spring of 1809, describing a series of experiments which I had just then finished on olefiant gas. It was read at the time to a committee of chemistry of the Royal Institution, but never published. If you consider it deserving of being published, it is much at your service, and it will be honoured by a place in your Journal. I give it without alteration as it was originally written. It belongs rather to that time than the present. The experiments were carefully made, though by a very young chemist, and even now, if I do not de-

ceive myself, some of the results may be worth preserving. They may derive, too, some interest from having been referred to by my brother, the late Sir Humphry Davy, in his *Elements of Chemical Philosophy*.—I am, my dear Sir, your obedient humble servant,

JOHN DAVY.

To Dr BREWSTER.

Chemists have long been divided by two opposite opinions respecting the heavy inflammable gases. By some it is supposed that an indefinite number of these compounds may exist. By others it is held that there are only two or three species, from the intermixture of which all other varieties are produced. This interesting subject has engaged the attention and exhibited the sagacity of some of our ablest philosophers; Cruickshank, Berthollet, Henry, and Dalton have done much to elucidate it. Very lately Dr Thomson has published a paper in confirmation of what has been already effected by the English inquirers, and announcing the discovery of a new gaseous compound of oxygen, hydrogen and carbon. Such a gas being singular, and my brother, Mr Davy, (the late Sir Humphry Davy,) conceiving that the facts from which its existence was inferred admitted of another interpretation, requested me to repeat Dr Thomson's experiments, making use of oxymuriatic or chlorine gas to separate the hydrogen and potassium, to ascertain whether carbonic oxide was present. Thus engaged, I was led from one experiment to another, into a particular inquiry respecting olefiant gas, the results of which are offered in the following pages.

It has, I believe, been hitherto taken for granted, that the gas produced by heating alcohol mixed with twice or thrice its bulk of strong sulphuric acid is pure olefiant gas. *A priori* there is no reason for this supposition. Besides olefiant gas, it is well known, that at certain periods of the operation, particularly towards its conclusion, sulphureous acid gas is produced in abundance; and I have always observed it accompanied by a proportional quantity of carbonic oxide. And as I found by a careful examination of the products, that sulphureous acid gas was evolved during the whole operation, very little indeed at first, but gradually increasing as the charcoal was precipitated

and the heat raised, it appeared probable that carbonic oxide was likewise formed at the same time. To acquire some certain knowledge respecting this, and to ascertain whether common olefiant gas contains any other impurity, I had recourse to chlorine gas.

The olefiant gas intended to be tried appeared perfectly good, it was the first portion collected after the common air of the retort had been expelled ; it had previous to its examination stood a few hours over lime-water ; it produced no diminution with nitrous gas, and it burnt with a bright yellow flame.

100 measures of this gas added to 110 measures of chlorine gas over mercury immediately formed the peculiar oil-like fluid, and the two gases entirely condensed each other with the exception of 20 measures, which on being transferred to water were reduced to 8 measures. The electric spark passed through the residue with an equal volume of chlorine gas, did not occasion inflammation, but the chlorine gas being absorbed by water and oxygen added, it produced an explosion, and the quantity of oxygen expended, and carbonic acid gas formed, showed that the residue was nearly pure carbonic oxide. With the chlorine gas used there were 2 measures of common air ; hence the gas examined consisted of 90 per cent of pure olefiant gas, and 10 carbonic oxide.

I have made several trials of this kind of different specimens of olefiant gas, and have never met with one purer than the last. I have sometimes detected the presence of common carburetted hydrogen in the residue in addition to carbonic oxide ; and I have always found common air after the gas had been kept over water for some time, though in the first instance it did not contain the smallest quantity. This last remark has, I believe, been several times made. Mr Dalton first ascertained that water absorbs one-eighth of its bulk of olefiant gas ; an important fact to those engaged in the analysis of this substance. I endeavoured by means of the easy absorption of olefiant gas by water to ascertain its impurities, but without success ; the residue I found to be greater than when chlorine gas was used for the same purpose, and to contain a larger quantity of common air owing, doubtless, to the expulsion of part of the air of the water by the olefiant gas dissolved.

Before I proceed farther, I think it necessary to remove one objection that I am aware may be made to the use of chlorine gas for determining the purity of common olefiant gas. There is a probability, it may be said, that the residual carbonic oxide is not mixed with the olefiant gas, but that it is produced by the agency of the chlorine gas. If this is the case, it must depend on the decomposition of water,—and muriatic acid gas must be formed as well as carbonic oxide. I have made the experiment over recently boiled mercury with dried gases, and could never observe the slightest traces of muriatic gas, though there was the usual residue of carbonic oxide. I have carefully weighed olefiant gas, making use of the very delicate balance belonging to the Royal Institution. It was mixed with about 10 per cent. of carbonic oxide, and it had been previously exposed to the action of sticks of caustic potash for five hours over mercury. The barometer during the whole time of the experiment was at 29.45, and the thermometer of Fah. at 52°.

The weight of an exhausted flask	-	= 444.5 grs.
full of common air,	-	= 444.5 + 9.4
again exhausted,	-	= 444.5
full of olefiant gas (30 cubic inches entered)	-	= 444.5 + 9.2
again exhausted	-	= 444.5

The proper corrections being made for the carbonic oxide present,* and for the mean temperature and pressure of 30 and 60, 100 cubic inches of olefiant gas are equal to 30.72 grs.

I have endeavoured to make an analysis of olefiant gas by firing it with oxygen in Volta's eudiometer both over mercury and water. But the results of my experiments were not sufficiently uniform to be quite satisfactory. I observed all those causes of ambiguity and inaccuracy pointed out by Dr Henry in the *Transactions of the Royal Society* for 1808. The principal sources of error appeared to me to exist in the ready absorption of olefiant gas by water and the simultaneous expulsion of air from the water; in the absorption and loss of part

* I have taken Mr Cruickshank's estimate of the specific gravity of carbonic oxide, and have made the corrections for difference of pressure and temperature in the usual way.

of the carbonic acid gas at the moment of its formation ; in the escape of some part of the charcoal unburnt ; and lastly, and chiefly, in the necessity of operating upon very small quantities. Despairing, therefore, of success in this way, I endeavoured to find out other modes of analysis.

With this in view, I first had recourse to sulphur for the purpose : and I was pleased to find, that, by subliming this substance in the olefiant gas in a small glass retort over mercury by the heat of a spirit-lamp, the decomposition of the gas was easily and expeditiously effected, sulphuretted hydrogen being formed and charcoal precipitated. *

The following table contains the results of my experiments. Sicilian sulphur or sulphur distilled from iron pyrites in vacuo was used, neither of which afforded any gas when heated in carbonic acid gas. The sulphuretted hydrogen formed was absorbed by water and its quantity thus ascertained.

	Measures of inflammable gas.	Of sulphuretted hydrogen formed.	Of residual gas undecomposed.	Of olefiant gas decomposed.
1.	69	69	34	35
2.	50	46	27	23
3.	54	55.5	26	28
1.	36	67	4	32
2.	48	92	3	45
3.	41	90	4	37

I examined the residual gas not absorbed by water after the separation of the sulphuretted hydrogen, and found it by means of chlorine and oxygen gas to be in the three first instances olefiant gas with the original impurity of the whole quantity of gas used, and in the others nearly pure carbonic oxide.

There is a remarkable difference between the three first and the three last results. The former were procured by using a quantity of sulphur not sufficient to saturate the whole of the hydrogen of the compound gas : or, by stopping the process before the whole of the olefiant gas was decomposed ; and the latter were obtained by means of sulphur in excess and a long

* Dr Austin found carburetted hydrogen procured from acetate of potash by heat to be decomposed by sulphur, and the Dutch chemists ascertained that there is a decomposition of olefiant gas produced by passing it over sulphur fused in a porcelain tube.

continuation of the heat : and in these experiments, besides the larger proportion of sulphuretted hydrogen, a little fluid, which did not appear in the three first, was produced, something similar in taste and smell to the liquor of Lampadius, (sulphuretted of carbon.)

It is evident, as gas out of the same jar was used in all these experiments, that the different proportions of sulphuretted hydrogen formed in the two sets, do not depend upon any variation of the proportion of hydrogen in olefiant gas, but are owing to the decomposition of the sulphur or charcoal by their reaction on each other. I am not sufficiently acquainted with the phenomena to venture to give an explanation of this very curious fact. I shall therefore confine myself to the second set of experiments, the results of which, I conceive, are not the less to be relied on, on account of their being associated with the preceding anomaly.

It is well known that the hydrogen in sulphuretted hydrogen is of the same density as in its uncombined state ; consequently, the results of the first three experiments prove that the proportion of hydrogen in olefiant gas is such, that, when expanded to its usual density by decomposition, it is equal to twice the volume of the olefiant gas decomposed, so that 100 measures of olefiant gas will afford 200 measures of hydrogen.

The weight of olefiant gas being known, we are thus in possession of data for calculating the proportions of hydrogen and charcoal constituting olefiant gas. 200 cubic inches of hydrogen gas, according to my brother's estimate of its specific gravity, weigh 4.384 grains, (barometer 30, thermometer 60,) this subtracted from 30.72 grains, the weight of 100 cubic inches of olefiant gas, leave 26.336 grains, the weight of the charcoal.

100 cubic inches, therefore, of olefiant gas (barometer 30, thermometer 60,) contain

4.384 grains	Hydrogen.
26.336	Charcoal.
30.720	Olefiant gas.

According to the very elaborate experiments of Messrs Al-

len and Pepys, 100 grains of carbonic acid gas consist of 28.6 grains of charcoal, and 71 oxygen; and the same gentlemen found 100 cubic inches of oxygen gas to weigh 34.2 grains. Consequently, 26.336 grains of charcoal, when saturated with oxygen, will form 192 cubic inches of carbonic acid gas.

To ascertain the proportion of charcoal in olefiant gas by direct experiment, I have made use of hyperoxymuriate of potash, red precipitate, and litharge. These substances heated by a spirit-lamp in the gas, in a small green glass retort, over mercury, are deprived, at least in part, of their oxygen, and water and carbonic acid gas are formed. With red precipitate and litharge the decomposition of the inflammable gas proceeds slowly; but with the hyperoxymuriate it is rapidly effected, so that inflammation is produced, provided there is some additional pressure of mercury to prevent the gas from being too much rarified by the heat. The following table contains the results of three experiments, selected from many others I have made of a similar kind:—

	Measures of inflam- mable gas.	Do. of unalter- ed resi- due.	Do. of oxy- gen pro- duced.	Do. of car- bonic acid gas form- ed.	Do. of olefiant gas de- composed.
Litharge,	40	30		20	10
Red precipitate,	30	23	7	14.5	7
Hyperoxymuriate,	16	10	6	12.25	6

The free oxygen produced having been separated by nitrous gas and green sulphate of iron, I found the residual gas merely common olefiant. It was entirely condensed by chlorine gas, with the exception of its original admixture of carbonic oxide.

As the results in the above table harmonize with the results of calculation, the one may be considered as a confirmation of the other, and we may conclude, therefore, with additional confidence, that the proportions of the constituents of olefiant gas are those already stated, viz. that the proportion of hydrogen is such that when expanded to its natural density it is double the volume of the olefiant gas, and the proportion of charcoal such that it forms when combined with oxygen a volume of carbonic acid gas equal to that of the expanded hydrogen.

It is well known that a considerable expansion, and that a precipitation of charcoal takes place when olefiant gas is inflam-

ed with rather less than its own bulk of oxygen. Mr Dalton, in his "*New System of Chemical Philosophy*," is of opinion, that all the oxygen in this instance is attracted by the charcoal, and that, consequently, the residual gas is a mixture of carbonic oxide and hydrogen. Dr Thomson, on the contrary, concludes from his own experiments, that it is a compound of oxygen, hydrogen, and carbon, and calls it oxycarburetted hydrogen, a new gas, he remarks, "differing in its properties from all other inflammable gases hitherto examined."

I have collected in the following table the results of some of the experiments I have made on this subject :—

Measures of olefiant gas.	Do. of oxygen.	Do. of residue after detona- tion.	Do. unabsorb- ed by lime water.	
5	4	16	16	No apparent precipi- tation of charcoal.
4	2			No explosion.
4	3	11	10.5	No charcoal precipit.
8	6.5	24.5	23	Charcoal precipitated.
16	10.5	30	26	Charcoal precipitated.

There appears to have been in all Dr Thomson's experiments a production of carbonic acid gas and a residue of oxygen. The latter using nitrous gas for the purpose, I could never detect, and the small diminution which I have observed on agitating the gas with lime water, I am inclined to attribute to the absorption of a little carbonic oxide, hydrogen, or perhaps remaining olefiant gas, rather than to that of carbonic acid gas, the formation of which in the present circumstance is to me unaccountable. And I am strengthened in this conclusion, by finding that less oxygen is required for the combustion of a given quantity of olefiant gas, when successive detonations are made with two small portions of oxygen, though there is no apparent precipitation of charcoal than when sufficient oxygen is used, and the whole of the inflammable gas is burnt at one operation. Hence I am induced to be of Mr Dalton's opinion, and to consider the oxycarburetted hydrogen of Dr Thomson as a mixture merely of hydrogen gas and carbonic oxide, the properties of which, I find, it possesses. Like a mixture of these gases, it burns with a white bluish flame, and detonated with chlorine gas it affords muriatic acid gas and a residue of carbonic oxide.

ART.V.—*Notice concerning an Autograph Manuscript by Sir Isaac Newton, containing some Notes upon the Third Book of the Principia, and found among the Papers of Dr David Gregory, formerly Savilian Professor of Astronomy in the University of Oxford.* By JAMES CRAUFURD GREGORY, M. D., F. R. S. E. Fellow of the Royal College of Physicians of Edinburgh.

AN opinion has been entertained by some of the modern French philosophers, and, among others, by the late celebrated Marquis de La Place, that it was only when far advanced in years that Sir Isaac Newton turned his attention to the study of theology; and it has been lately assumed by M. Biot, in an account of Newton and his discoveries, contained in the "*Biographie Universelle*," as a fact which can scarcely be doubted, that, at one period of his life, he was actually in a state of mental derangement.

The only evidence adduced in support of this remarkable assertion, is the following note, said to have been written by Huygens, and communicated to M. Biot by M. Van Swinden: "*Die 29 Maii 1694.*—Narravit mihi D. Colin, Scotus, virum celeberrimum ac rarum geometram Isaacum Newtonum incidisse in phrenitin abhinc anno et sex mensibus. An ex nimia studii assiduitate, an dolore infortunii, quòd in incendio, laboratorium chemicum et scripta quædam amiserat? Cùm ad Archiepiscopum Cantabrigiensem venisset, ea locutum quæ alienationem mentis indicarent; deinde ab amicis cura ejus suscepta, domoque clausa, remedia volenti nolenti adhibita, quibus jam sanitatem recuperavit et jam nunc librum suum Principiorum intelligere incipiat." Upon this note the following observations are made by M. Biot:—"Il paraît d'après ces détails que l'on ne saurait guère douter du fait même, c'est à dire, que cette tête qui pendant tant d'années s'était appliquée continuellement à des contemplations si profondes quelles étaient comme la dernière limite de la raison humaine, se serait enfin troublée elle-même par l'excès de ses efforts, ou par la douleur d'en voir les résultats anéantis."

The misfortune here alluded to by Huygens and M. Biot, is the well known anecdote of the loss caused by his dog Dia-

mond ; and it is to this circumstance principally that M. Biot ascribes the mental derangement under which he supposes Newton to have laboured. He adds, "Mais ce fait, d'un dérangement d'esprit, quelle qu'en puisse être la cause, expliquerait pourquoi, depuis la publication du livre des Principes en 1687, Newton âgé seulement alors de 45 ans, n'a plus donné de travail nouveau sur aucune partie des sciences, et s'est contenté de faire connaître ceux qu'il avait composé long-temps avant cette époque, en se bornant à les compléter dans les parties qui pouvaient avoir besoin de développemens." M. Biot supposes, that after this period of Newton's life, he almost ceased to think on scientific subjects, and that religious reading formed his most habitual occupation ; and he states, that after the "fatal epoch," as he terms it, of 1693, only three really new scientific productions appeared from his hand, one of which had been probably prepared for a long time previously, and the others had cost him very little time.

It is not the object of the present notice to disprove the assertion that Sir Isaac Newton was at one period of his life in a state of mental derangement ; but to show on what slender grounds this allegation rests, it may be mentioned, that M. Biot seems to find an argument in its favour in the magnanimity with which Newton bore the irreparable loss of the labour of many years ! And, as a proof that his mental powers were not impaired, either by this accident, or by the advance of years, it may be sufficient to allude to the fact, admitted by M. Biot himself, that, in the year 1716, and at the age of 74, Newton, on returning from his duties at the Mint, and when much fatigued with the business of the day, solved, before he retired to rest, the celebrated problem of Orthogonal Trajectories, proposed by Leibnitz with a view to prove the superiority of his calculus over Newton's method of fluxions, and, as he expresses himself, "ut pulsum Anglorum Analystarum nonnihil tentemus." *

As Sir Isaac Newton's *Observations on the Prophecies and the Apocalypse* were not published during his lifetime, and as the celebrated general Scholium at the end of the *Principia*

* Leibnitii et Bernoullii *Commercium Epistolicum*, tom. ii. Epist. ccxxvi. p. 365.

only appeared in the second edition of that work, published in 1713, when the author was in his 71st year, the supposition entertained by La Place, that Newton only turned his attention to the subject of theology in his very advanced years, is somewhat more natural and plausible in the absence of any direct evidence to the contrary. So impressed was he with this idea, that M. Gautier, Professor of Astronomy in Geneva, mentioned, when in this country a few years ago, that he had been commissioned by La Place to make inquiries on this subject. Any opinion entertained by this great man, by whose genius and labours the Newtonian Philosophy may be said to have been completed in all its details, is entitled to due respect and attention from all, and, in the minds of many, must carry with it considerable weight. A document in my possession, however, appears to me to furnish clear evidence that Newton had formed the theological opinions expressed in the Scholium already mentioned, at least fifteen years before the publication of the edition of the *Principia*, in which it first appeared; and it has occurred to me, that a short statement of this evidence, in opposition to the opinion entertained by La Place, might not be unacceptable to the Royal Society.

Several years ago, in looking over some manuscript mathematical papers which belonged to David Gregory, Savilian Professor of Astronomy in the University of Oxford, the contemporary and intimate friend of Newton, I found (along with several other autograph fragments on mathematical subjects) one manuscript, consisting of twelve folio pages, in the handwriting of Newton, and containing, in the form of additions and scholia to some propositions in the Third Book of the *Principia*, an account of the opinions of the ancient philosophers on gravitation and motion, and on natural theology, with various quotations from their works.

It appears from this manuscript, that Newton was not only well acquainted with the opinions and reasonings of the ancients upon these subjects, but that he has done ample justice to their sagacity. This is a point which it is of some importance to have ascertained, as it has been asserted, particularly by M. Dutens, in his "*Recherches sur l'Origine des Découvertes attribuées aux Modernes*," published in 1766, that the

modern philosophers supposed the principle of universal gravitation to have been quite unknown to the ancients, and had therefore claimed the merit of the first discovery. The authorities and quotations adduced by M. Dutens to prove that the general principles of motion and gravitation were known to the ancients, are precisely the same as those contained in Newton's manuscript, a considerable part of which, I find, had been long before published, nearly verbatim, in the preface to the "*Astronomiæ Physicæ et Geometricæ Elementa*" of David Gregory. The passage from Lucretius, brought forward by M. Dutens as a proof that the resistance of the medium through which they pass, was known to the ancients to be the cause of the difference in the velocity with which bodies fall, is quoted at full length by Newton after the following remarks:—"Et quamvis res leviores quæ aëris vel aquæ resistantiam difficilior vincunt in his fluidis descendant tardius, tamen in spatio vacuo ubi nulla est resistantia, atomos omnes tam graviore quam minus graves propter gravitatem sibi proportionalem æquali celeritate descendere, sic docet Lucretius." And the two remarkable lines,

" Omnia quapropter debent per inane quietum,
Æque ponderibus non æquis concita ferri,"

are written in large characters, with a view, evidently, to show the importance which he attached to them.

This account of the opinions of the ancients occupies the greater part of the manuscript; but attached to it there are three very curious paragraphs. Two of these appear to have been the first draught of the general Scholium at the end of the edition of the *Principia*, published in 1713, and express the same theological opinion. It is remarkable, however, that it is only in the third edition, published in 1726, (the year before that in which Newton died,) that the substance of the second of these paragraphs is found.

The first paragraph expresses nearly the same ideas as some sentences in the Scholium, commencing—"Deus summus est ens, æternum, infinitum, absolute perfectum :". The first part of it is as follows, and the expressions appear to me still more striking and sublime than those in the Scholium itself:—"Deum esse ens summe perfectum concedunt omnes. Entis autem

summe perfecti Idea est ut sit substantia una, simplex, indivisibilis, viva et vivifica, ubique semper necessario existens, summe intelligens omnia, libere volens bona, voluntate efficiens possibilia, effectibus nobilioribus similitudinem propriam quantum fieri potest communicans, omnia in se continens tanquam eorum principium et locus, omnia per præsentiam substantialem cernens et regens (sicut hominis pars cogitans sentit species rerum in cerebrum delatas, et illinc regit corpus proprium,) et cum rebus omnibus, secundum leges accuratas ut naturæ totius fundamentum et causa, constanter cooperans, nisi ubi aliter agere bonum est." (See Note A.)

The second paragraph expresses precisely the same idea as the sentences of the Scholium, in the edition of 1726, beginning, "A cæca necessitate metaphysica, quæ utique eadem est semper et ubique, nulla oritur rerum variatio;" and is as follows:—"Quicquid necessario existit illud semper et ubique existit, cum eadem sit necessitatis lex in locis et temporibus universis. Et hinc omnis rerum diversitas, quæ in locis et temporibus diversis reperitur; ex necessitate cæca non fuit, sed a voluntate entis necessario existentis originem duxit. Solum enim ens intelligens vi voluntatis suæ, secundum intellectuales rerum ideas, propter causas finales, agendo, varietatem rerum introducere potuit. Varietas autem in corporibus maxime reperitur, et corpora quæ in sensus incurrunt sunt Stellæ fixæ, Planetæ, Cometæ, Terra, et eorum partes."

The third paragraph relates to the same subject as the last paragraph of the Scholium, in which, as in his *Optics*, it is well known that Newton favours the hypothesis of a subtile and universally pervading Æther. But it is singular that it expresses upon this subject an opinion different from, and perhaps some may think sounder than, that which was afterwards published. This paragraph begins as follows:—"Coelos et spatium universum aliqui materia fluida subtilissima implent, sed cujus existentia nec sensibus patet nec ullis argumentis convincitur, sed hypotheseos alicujus gratia præcario assumitur. Quinimo si et rationi fidendum sit et sensibus, materia illa e rerum natura exulabit;" and then proceeds to give reasons for this opinion, of the validity of which I do not pretend to judge. (See Note B.)

This manuscript bears no date, but two circumstances enable me to state that it must have been written certainly eleven, and in all probability fifteen, years before the publication of the second edition of the *Principia* in 1713. 1st, The edition of David Gregory's *Elements of Astronomy*, into which, as already stated, much of that portion of the manuscript which relates to the opinions of the ancients has been transferred almost verbatim, was published in the year 1702. 2d, I find the whole of the manuscript fairly copied in the handwriting of David Gregory, into the end of a manuscript book, containing his unpublished notes upon the *Principia* of Newton, and bearing a running date from 1687 to 1697. In 1702 Sir Isaac Newton was not more than 60 years of age, and if, as appears almost certain, David Gregory received this manuscript from him between 1687 and 1697, he must have received it when Newton was between 45 and 55 years of age.

I do not know whether it is true, as stated by Huygens, "Newtonum incidisse in Phrenitin;" but I think every gentleman who examines this manuscript will be of opinion that he must have thoroughly recovered from his Phrenitis before he wrote either the commentary on the opinions of the ancients, or the sketch of his own theological and philosophical opinions which it contains.

Since the foregoing notice was read, some additional light has been thrown on the points to which it refers, by the publication of Sir Isaac Newton's Correspondence with Mr Locke, in the Life of the latter by Lord King. Two of Newton's letters to Locke, contained in this valuable work, might, at first sight, appear to favour the supposition of a temporary derangement in his intellect, more especially as they correspond in point of date with M. Biot's "fatal epoch" of 1693; and as Newton himself states in one of them, that he had totally forgotten what he had written on the subject of Locke's doctrine concerning innate ideas, scarcely three weeks before, in the other. (See Note C.)

But I believe it will be found, on closer examination, that these letters, unsupported by more direct evidence, will scarcely warrant so harsh and uncharitable a conclusion. It appears

from his own statement to Locke, that during the early part of the year 1693, Newton laboured under some bodily indisposition, attended with loss of sleep for a considerable time; and that his health had farther suffered by a disorder which had been epidemical during the summer of that year. The state of mental exhaustion, and probably of nervous irritability, brought on by this disorder of his health and want of rest, would, of itself, I conceive, go far under any circumstances to account for what it might otherwise appear difficult to explain in these letters. But when we take, at the same time, into consideration, that Newton, with all his boldness of conception and his extraordinary sagacity, was a man of a peculiarly timid and apprehensive character, and perhaps suspicious temper, or as Locke (certainly a very competent judge) expresses it, "a nice man to deal with, and a little too apt to raise in himself suspicions where there is no ground," (See Note D;) I think we can be at no loss to find a satisfactory explanation of these, as well as other letters he may have written during this period, without having recourse to the very improbable and gratuitous assumption of an idiopathic derangement of his intellect. Not only is there no direct evidence to support this assertion, but there is abundant proof that, during the alleged period of his insanity, he was employed, with all his characteristic vigour of mind and patient reach of thought, upon various abstruse and profound investigations. Among these may be mentioned the celebrated letters to Dr Bentley on the Existence of a Deity, which, though only published in 1756, were all written in 1692 and 1693.

Mr Dugald Stewart, who had an opportunity of reading Newton's letters to Locke, some years before they were published, saw nothing in them but "a humility and candour worthy of himself," and "an ingenuous and almost infantine simplicity." And, with respect to the opinion which Newton acknowledges he had entertained as to the tendency of Locke's reasonings against innate ideas, Mr Stewart states, that he appears to have felt precisely in the same manner with Lord Shaftesbury, the author of the *Characteristics*, who, in his remarks on this subject, appears to Mr Stewart "to place the question about *innate ideas* upon the right and only philosophical footing; and

to afford a key to all the confusion running through Locke's argument against their existence."

That Locke, who was intimately acquainted with Newton at the period of 1693, as well as for several years before and many afterwards, saw nothing in his conduct or correspondence to induce him to infer that he was at that time labouring under any degree of mental derangement, is evident from his requesting Newton, as a favour, to point out those passages in his essay which had appeared to him objectionable on account of their tendency, in order that he might correct them and explain himself better. Had there been any reasonable foundation for the charge of insanity, it is not likely that it would have escaped the penetration of so attentive and accurate an observer of the human intellect.

The publication of Newton's correspondence with Locke, throws farther light also on the period of his life at which he turned his attention particularly to theology and the study of the Scriptures. It appears clearly from Newton's letters, that they corresponded on the subject of the Prophecies of Daniel, so early as the year 1691; and that the "Historical Account of Two Notable Corruptions of the Scriptures," which M. Biot supposes Newton to have written about the year 1712 or 1713, when he was upwards of 70, was actually composed and transmitted to Locke (by whom it was forwarded to Le Clerc for the purpose of being translated into French) in the year 1690, prior to the alleged period of his insanity, and when he was only 48 years of age.—*Trans. Royal Soc. Ed.*

(Note A).—The remainder of this paragraph is as follows: "Nam liberrime agit quæ optima et ratione maxima consentanea sunt, et errore vel fato cæco adduci non potest ut aliter agat. Hæc est idea Entis summe perfecti, et conceptus durior Deitatem minime perficiet, sed suspectam potius reddet, aut forsân excludet e rerum natura."

(Note B).—The rest of this paragraph is as follows:—"Nam quomodo motus in pleno peragatur intelligi non potest; cùm partes materiæ, utcunque minutæ, si globulares sint, nunquam implebunt spatium solidum; sin angulares, propter omnimodum superficierum contactum firmitus hærebunt inter se quàm

lapides in acervo, et ordine semel turbato, non amplius congruent ad spatium solidum implendum. Porro tam experimentis probavimus quàm rationibus mathematicis, quod corpus sphaericum densitatis cujuscunque in fluido ejusdem densitatis utcunque subtili progrediens, ex resistantia medii prius amittet semissem motus sui quam longitudinem diametri suæ descripserit. Et quod resistantia fluidi illius nec per subtilem partium divisionem, nec per motum partium inter se diminui possit, ut corpus longitudinem diametri prius describat quam amittat semissem motus."

(Note C.)—To enable the reader to form his own opinion, these two letters are here subjoined. They are both addressed to Mr Locke.

"SIR,—Being of opinion that you endeavoured to embroil me with women and by other means, I was so affected with it, as that when one told me you were sickly and would not live, I answered, 'Twere better if you were dead. I desire you to forgive me this uncharitableness. For I am now satisfied that what you have done is just, and I beg your pardon for my having hard thoughts of you for it, and for representing that you struck at the root of morality in a principle you laid down in your book of Ideas, and designed to pursue in another book; and that I took you for a Hobbist. I beg your pardon also for saying or thinking that there was a design to sell me an office, or to embroil me. I am your most humble, and unfortunate Servant,

IS. NEWTON."

"At the Bull, in Shoreditch,
London, Sept. 16, 1693."

In answer to these painful acknowledgments, Locke, in a letter written, as Mr Stewart justly remarks, "with the magnanimity of a philosopher, and with the good-humoured forbearance of a man of the world," requests Newton to point out the places in his book that gave occasion to his censure, in order that, by explaining himself better in a second edition, he may avoid being mistaken by others, or unawares doing the least prejudice to truth or virtue. Newton's reply is as follows:

"SIR,—The last winter, by sleeping too often by my

fire, I got an ill habit of sleeping ; and a distemper which this summer has been epidemical, put me farther out of order, so that when I wrote to you, I had not slept an hour a night for a fortnight together, and for five nights together not a wink. I remember I wrote to you, but what I said of your book I remember not. If you please to send me a transcript of that passage, I will give you an account of it if I can. I am your most humble Servant,

Is. NEWTON."

" *Cambridge, Oct. 5, 1693.*"

Note D.—These remarkable expressions occur in a very curious and interesting letter also contained in Lord King's *Life of Locke*. As it relates to the character of Newton, and appears to me to throw considerable light on the subject under discussion, I shall make no apology for inserting it here, especially as it seems to be but little known. It is a confidential letter from Locke to his friend and relation Mr King, afterwards Lord Chancellor ; and it can scarcely be doubted that, had there been any truth in the supposition of Newton's previous derangement, he would have made some allusion to it on this occasion.

" DEAR COUSIN,

Oates, April 30, 1703.

" I am puzzled in a little affair, and must beg your assistance for the clearing of it. Mr Newton, in autumn last, made me a visit here ; I showed him my essay upon the Corinthians, with which he seemed very well pleased, but had not time to look it all over, but promised me if I would send it him, he would carefully peruse it, and send me his observations and opinion. I sent it him before Christmas, but hearing nothing from him, I, about a month or six weeks since, writ to him, as the inclosed tells you, with the remaining part of the story. When you have read it and sealed it, I desire you to deliver at your convenience. He lives in German Street : You must not go on a Wednesday, for that is his day for being at the Tower. The reason why I desire you to deliver it to him yourself is, that I would fain discover the reason of his so long silence. I have several reasons to think him truly my friend, but he is a nice man to deal with, and a little too apt to raise in himself suspicions where there is no ground ; therefore, when

you talk to him of my papers, and of his opinion of them, pray do it with all the tenderness in the world, and discover, if you can, why he kept them so long, and was so silent. But this you must do without asking why he did so, or discovering in the least that you are desirous to know. You will do well to acquaint him that you intend to see me at Whitsuntide, and shall be glad to bring a letter to me from him, or any thing else he will please to send; this perhaps may quicken him, and make him dispatch these papers if he has not done it already. It may a little let you into the free discourse with him, if you let him know that when you have been here with me, you have seen me busy on them (and the Romans too, if he mentions them, for I told him I was upon them when he was here,) and have had a sight of some part of what I was doing.

“Mr Newton is really a very valuable man, not only for his wonderful skill in mathematics, but in divinity too, and his great knowledge of the scriptures, wherein I know few his equals. And therefore pray manage the whole matter so as not only to preserve me in his good opinion, but to increase me in it; and be sure to press him to nothing, but what he is forward in himself to do.”

ART. VI.—*A Description of a New Construction of Sir Isaac Newton's Microscope.* By R. POTTER, Esq. Junior. Communicated by the Author.

It is well known to all who have had experience in practical optics, that the more complex their instruments, the less likely are they to perform their office well, from unavoidable errors in workmanship, and imperfections in the materials. And thus it is generally said, that by far the greater number of microscopic discoveries have been made with the single lens or the small globules. Amongst compound microscopes, the reflecting microscope of Sir Isaac Newton stands next in simplicity to the single lens, having only one additional surface necessary to the instrument; and we might reasonably expect, that, under these circumstances, it would prove a useful tool in the hands of the scientific inquirer.

In this construction the object is placed directly in the fo-

cus of the speculum, and the image is formed in that of the eye-glass.* The difficulty of giving a sufficient illumination to the object has been the principal fault ascribed to this plan; but I have found that there is no difficulty in this respect. My first construction was for opaque objects, to illuminate which, I cut a large circular hole in the tube betwixt the object and the speculum, as at *a b c* Plate I. Fig. 1 and 2, but a good deal of indistinctness then arose in the field of view, from the irregular light which fell on the sides of the tube. This defect I, however, soon remedied completely, by lining all the lower parts of the tube with black velvet, as the most unreflecting substance known. With this lining the image appears projected on a black ground, and exhibiting the natural colours of the objects. I generally employ a large lens, as at *d*, to concentrate the illumination on those objects which are better observed by such a light, than by one falling upon them in various directions.

I believe this construction to show all opaque objects in a manner superior to any other microscope, and that it will show some which no other will show.

For transparent objects I have applied a lens also, to concentrate the light upon the object, as at *e*. The pencil of rays passing through the lens falls upon a small diagonal mirror in the axis of the tube, as shown in the figure, and set at an angle of 45 degrees. By this plan a very strong light may be thrown through and aside the object. The illuminating lens ought to be so mounted that it can be adjusted for the focus of the light employed to fall exactly on the object. And by moveable caps to cover the aperture *a b c*, and the lens *e*, the interference of all foreign light is prevented; whilst the opaque and transparent methods may be successively employed without altering the position of the object, which is an advantage every naturalist will duly appreciate.

The objects I keep attached to thin brass pins stuck into

* It is the same microscope that I mentioned in my paper on speculum polishing, &c. as particularly effective on opaque objects; but having at that time had occasion to consult Priestley's *History of Vision*, I was misled by an erroneous passage there, to attribute the first proposal of the plan to Barker, (as it should have been written.)

small wooden handles, as shown at *h* in the figure, these pins pass through a slit cut into a small piece of cork, which is attached to the sliding piece *g*. This sliding piece carries at the same time the lens *e* and the plane mirror; and the whole are moved together by the small arm connected to the crank as at *i*. A nut attached to the pivot on which the crank is fixed serves when turned to adjust the object to the focus.

The advantages of the construction are its superior distinctness, and large quantity of light; the former from there being only one reflection between the object and the image, and the latter from our being able with an ellipsoidal figure, (which I have found the means of giving with a very considerable degree of certainty and correctness,) to have a mirror of very large diameter, compared with its focal length.

The greatest disadvantage in the instrument is in the greater trouble required in keeping the objects, as it is necessary to have them attached to small pins. I have them placed in rows in a box where they are kept, without touching each other, and a considerable number may be thus kept in a little compass.

I do not expect this microscope ever to come into use as a toy instrument; but I certainly recommend the construction to naturalists, and those who use the microscope in scientific researches, with whom a small degree of extra trouble is lightly regarded, when they can obtain at the same time a much more powerful and distinct apparatus.

It will be always acknowledged a great advantage in any microscope, that it is applicable, like the one here described, to almost every description of objects. In the construction I use, I have a speculum of one inch diameter, and one and a-half inches focal length, and generally employ a distance of about ten to fourteen inches between the object and image. The size of one inch diameter of speculum allows me to place an insect, or other object, of one-fourth of an inch square, in the tube without any perceptible ill effect resulting; and it may thus be examined entire as an opaque object, or such parts as are transparent may, in the course of the examination, be viewed in the appropriate light, and this also with very high powers. But though large and bulky objects may be examin-

ed entire, the microscope has a paramount effect in critical defining power.

I am indebted to Dr Brewster for information on the necessity of having the focus of the illuminating lens for transparent objects to fall *exactly* upon the object, when great nicety of vision is required. Having adjusted my microscope carefully in this point, I saw quite easily what are called the diagonal lines on the scale from the wing of the white cabbage butterfly, which has been proposed as a difficult test-object by Dr Goring ; and it is such an one as those who have only seen the stronger longitudinal striæ on scales from the wings of moths and butterflies, have little idea of. To view this sort of object, I use small bits of talc, which, being *slightly* moistened with weak gum water, the small objects are made to adhere to it, and the whole are attached, as before, with gum water to the thin brass pins, and may be applied similarly to other objects. I have also some placed between two small pieces of talc ; but on careful comparison, I think the other plan, when used judiciously, to be the best.

The instrument shows me also easily, not only the striæ on the scales of the wing of the small house moth, but also the diagonal lines.

I have also seen a much more difficult object than these just referred to in the web of the spider, called the *Clubiona atrox*.* Mr Blackwall, who first noticed this singular web, has described it, and also the mode of its construction, in a paper read a short time ago before the Linnean Society, and which will be found in the forthcoming volume of their Transactions. It consists of a straight and strong line, upon which is attached a white and curved line, and the whole are surrounded by a broad bluish band. There can be no doubt that this blue band consists of lines produced by the spider, and woven into the delicate tissue. (See Mr Blackwall's arguments in the paper above-mentioned.) To demonstrate these fibres, however, is a work for an expert microscopist provided with a first-rate instrument. So critical a defining power is required at the same time with a large quantity of light, that I doubt much

* It is the spider found in the crevices of old walls, and may be known by its making an irregular fleecy-looking web.

whether any compound refracting microscope, even the best achromatic, will ever show the construction of this web as a transparent object. When viewed in this manner through good common compound microscopes, the blue band can scarcely be perceived at all with a moderately high power. It is better seen as an opaque object by the light of the sun, and it was on this method that I discovered it when highly illuminated and highly magnified to be covered very regularly and closely with white spots. This was sufficient information that it was of a uniform texture; but as there is always in such a light a strong display of irradiations and prismatic colours, it was impossible to trace the fibres. I had discovered something of the texture with small globules of glass, used after the manner prescribed by Lewenhoeek; but with very high powers the distinct field of view is so small that I dared hardly to pronounce decidedly upon the general structure; and it was only after adjusting the illuminating lens of my microscope very carefully, that I saw with it the complete structure of a regularly woven net. It will always, nevertheless, require a practised eye, and some acquaintance with the object, to see it well. I take a short length of the web for examination, carefully attached across a wire bent for the purpose, and fixed in a small handle as with other objects. When the webs become old they are white and opaque, and are in this state more fit for seeing the fibres; but if the general figure of the web is to be studied, it should be procured new, before it has got in any way deranged, and it has then a pale blue misty appearance.*
November 2d 1831.

ART. VII.—*On the production of Ammonia by the action of Sulphuretted Hydrogen on Nitric Acid.* By JAMES F. W. JOHNSTON, A. M. &c. &c. Communicated by the Author.

It is known from the observations of Priestley, Davy, and Austin, that, under certain circumstances, mixtures of nitrous gas (deutoxide of azote) and sulphuretted hydrogen, by a mutual interchange of their elements, produce ammonia and an acid of sulphur. I am not aware of any experiments which show the same thing in regard to sulphuretted hydrogen and liquid

* See article XI. in this Number.

nitric acid. The usual and well known effect of nitric acid upon sulphur in all its states is to oxidize it while the acid itself is degraded into one of the lower oxides of azote, and sometimes, though rarely, into pure azotic gas. Vogel made direct experiments on the action of concentrated nitric acid upon gaseous sulphuretted hydrogen, and his statement is, that the acid oxidizes the hydrogen and one portion of the sulphur,—the other portion being precipitated in flocks. Ammonia, however, is formed in considerable quantity; but this escaped him.

I had dissolved in nitric acid a quantity of the cobalt pyrites of Skyteryd, which had been already roasted at the cobalt works of Fossum in Norway, and subjected it to a rapid and prolonged current of sulphuretted hydrogen, to separate the remaining arsenic. The filtered solution was evaporated to dryness, redissolved, and left for crystallization. Besides the cobalt salt there was deposited a large crop of beautiful transparent regular octahedrons of a pale purplish tint. These on examination proved to be the sulphate of iron and ammonia, a salt which with considerable propriety has been classed by Dr Thomson among the alums.

It became a question, therefore, whence the ammonia was derived. The only source for it seemed to be in the hydrogen of the sulphuretted hydrogen and the azote of the nitric acid. The following experiments show this to be its true origin, and that the phenomenon can be reproduced at pleasure.

1. A portion of crystallized protosulphate of iron was dissolved in diluted nitric acid, and subjected to a current of sulphuretted hydrogen gas. The solution was rendered colourless, and sulphur was deposited. Being heated the iron was again per-oxidized, and by a second current of gas was again rendered colourless. It was filtered and evaporated, when a yellowish white salt remained with much free sulphuric acid. This salt does not readily dissolve; but if a little water be added, and the capsule set aside, regular octahedrons are gradually formed, possessing the beautiful purplish tint which the ammonia sulphate of iron possesses when crystallized from an acid solution.

2. About a quarter of an ounce of liquid nitric acid was diluted with an ounce of water, and subjected to a slow current of sulphuretted hydrogen continued for several hours. Sul-

phur was deposited, and the acid liquor remaining after evaporation gave, with excess of caustic potass, a decided smell of ammonia. The production of ammonia, therefore, is not due to the predisposing action of any third body,—as the iron, for example, in the first experiment,—but solely to the mutual action of the two compound bodies from the elements of which the ammonia derives its constituent parts.

3. To procure the ammonia in larger quantity, an ounce of liquid nitric acid was employed diluted with an equal bulk of water, and a rapid current of sulphuretted hydrogen passed through. In this case much heat was evolved, nitrous gas and sulphur vapour were given off, and a cake of sulphur was speedily deposited on the surface of the liquid. Filtered and evaporated till nitric acid ceased to be given off, a sulphuric acid solution remained, which on cooling showed the presence of an ammoniacal salt, by the presence of minute floating crystals, which were probably sulphate of ammonia, but could not be separated.

Caustic potass added in excess evolved ammonia, but the most elegant test for the alkali is the sulphate of iron. To one-half of the above residual liquid a solution of persulphate of iron was added,—the whole evaporated till nearly dry, and set aside with a little water. In a few days a large and beautiful crop of the alum crystals was obtained. This mode of detecting ammonia in acid solutions, from the great ease with which the crystals may be formed, may occasionally prove of considerable service in *qualitative* analysis. It may even be employed to determine the quantity of the alkali in solutions which contain no more fixed substance, and in certain cases where the weight of the fixed substances has been already determined, as the double salt loses its water and forms a dry whitish mass at a temperature considerably lower than that at which the sulphate of ammonia it contains begins to be decomposed.

4. Sulphuretted hydrogen passed through a cold concentrated solution of neutral nitrate of barytes is slowly but sensibly decomposed; the solution becomes troubled, and a deposit of sulphur and sulphate of barytes is formed. In a hot solution the effect is more speedy, and the precipitate more abundant; and if the solution be rendered acid by the addition

of nitric acid, the decomposition takes place with still greater ease and rapidity. The filtered solution evaporated to small volume, and treated with excess of caustic potass, emits the odour of ammonia. In this case, then, the sulphuric acid falling as it is formed in combination with the barytes, the ammonia remains in the solution in the state of a nitrate.

These observations are not unworthy the attention of the analytical chemist. They show that in all cases where sulphuretted hydrogen is caused to pass through solutions containing nitric acid either in a free or combined state, we may look for the formation of sulphuric acid and ammonia to a greater or less amount. When substances exist in solution, therefore, upon which either of these products may exercise a prejudicial influence, it will be necessary to guard against their production.

Solutions containing lead are generally acidulated with nitric acid, and often contain no other acid; and the formation of a portion of sulphate which falls along with the sulphuret when sulphuretted hydrogen is passed through it, may be one reason why the weight of the sulphuret cannot be depended on, and which renders the conversion of the whole into sulphate necessary. Were there no free sulphur present in the dry sulphuret, it is obvious that a very small quantity of sulphate would introduce an important excess of weight.

The same remark applies to solutions which contain barytes; and as sulphuretted hydrogen is generally employed to separate it from lead, a slight admixture of sulphate of barytes may increase the weight of the sulphuret of lead thrown down.

In regard to the ammonia, cases may occur where, if formed in any quantity, it may retain in solution a substance we are desirous to have entirely precipitated. Small portions of magnesia, or the oxides of manganese, cobalt, and nickel, may escape us, and introduce errors into our results.

But it is in the analysis of the nitrates, with a view to the correct determination of the nitric acid, that the formation of ammonia as well as of sulphuric acid is most likely to introduce errors. The mode recommended by Rose * for determining the amount of the nitric acid in the metallic nitrates, whether soluble or insoluble, which can be decomposed and precipitated

* *Manual of Analytical Chemistry*, translated by Griffin, part ii. p. 371.

by sulphuretted hydrogen gas, is to subject them either in solution or in the state of a fine powder mixed with the water, to the action of a current of the gas. A sulphuret is formed, the nitric acid is liberated, and remains in solution uncombined. The solution is filtered, mixed with hydrate of barytes in excess, and slowly evaporated to dryness. The excess of barytes attracts carbonic acid from the air, and is thus rendered insoluble. "The small quantity of sulphuret of barium produced by the excess of sulphuretted hydrogen is converted by oxidation first into hyposulphite, and finally into sulphate of barytes." The nitrate of barytes is dissolved out, the barytes precipitated by sulphuric acid, and from the weight of the sulphate that of the nitric acid is calculated.

Now, the above experiments show that the presence of free nitric acid in solution gives rise to the formation of ammonia. A portion of the acid, therefore, is decomposed by the sulphuretted hydrogen, and, consequently, the weight of acid obtained by the above process must always be defective. The formation of one grain of ammonia would cause a loss of upwards of three grains of nitric acid, for the atoms are to one another as 2.125:6.75. Besides, it is more than probable that the oxidation of the sulphuret of barium, as stated by Rose in the sentence above quoted from his book, is due also in part to the oxygen of the nitric acid, in which case another source of error also would be introduced.

In the details of mineral analysis, then, we shall run the smallest risk of error from the source adverted to in this paper. 1. By using nitric acid as a solvent as seldom as possible; 2. By acidulating solutions to be subjected to the action of sulphuretted hydrogen by as small an excess of nitric acid as we can; and 3. By continuing the current of gas no longer than is absolutely necessary. By these precautions the chances of error where they exist will be reduced within very narrow limits.

PORTOBELLO, 12th Nov. 1831.

ART. VIII.—*Addition to a Paper "On the Nature of the Light in the Two Rays produced by the Double Refraction of Quartz."* By G. B. AIRY, M. A.; M. G. S. Late Fellow of Trinity College; Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge; and Fellow of the Cambridge Philosophical Society. (Read April 18, 1831.)

AFTER I had made the experiments described in a paper which was read to this Society on February 11, 1831, I received from Mr Dollond an apparatus constructed under my direction, which for convenience and extent of application exceeds any other that I have seen. The parallel rays that fall on a piece of plate glass blackened at the back are reflected, all completely polarized, and are received on the first lens, which makes them all pass through one point; then diverging they are received on the second lens, whose distance from that point is equal to its focal length, and they emerge from it parallel. In this state they are received on the analyzing plate (a piece of plate glass blackened behind) and all are of course completely analyzed. They are then received on the third lens, (fixed in a sliding tube, like the eyeglass of a telescope) which makes them all pass through the eyehole. The lenses are of equal focal length, and their arrangement is precisely the same as that of the lenses in the old three-glass eye-piece, measuring the distance between the second and third lenses along the incident and reflected ray. The analyzing plate, with the second and third lenses, turns on a spindle parallel to the rays polarized by the first plate, whose direction passes through the centers of the first and second lenses. To see the rings, &c. produced by a crystal, it should be placed at the point where the rays cross between the first and second lens; and a specimen $\frac{1}{20}$ inch broad so placed will show the rings with perfect brilliancy and clearness in their utmost extent. If it is wished to use a micrometer, the micrometer should be placed between the polarizing plate and the first lens, at a distance from the latter equal to its focal length, where it will be seen distinctly at the same time that the rings are seen distinctly. If it is wished to see the maced structure

of quartz, amethyst, topaz, &c. the crystal is to be placed in the situation assigned for the micrometer: then no rings will be seen, but on turning the analyzing apparatus round its spindle the different parts will be differently coloured. For the use of plane-polarized light only, there is no need for a polarizing plate larger than the projection of the first lens: and the distance between the nearest edge of the polarizing plate and the first lens needs not to exceed by a large quantity the focal length of the latter. But as I now consider every apparatus incomplete which does not allow of the use of circularly and elliptically polarized light, I have had the polarizing plate separated so far from the lens that it allows Fresnel's rhomb to be placed between them, room being left for the micrometer, &c. between the rhomb and the lens: and the polarizing plate is made so large that it will transmit plane-polarized light to the end of the rhomb, at whatever angle it is placed, while the other end is centrally opposite to the first lens.

To use artificial light with facility, a lamp is placed in the focus of a lens whose diameter is equal to the diameter of the circle described by the point of the rhomb. The board which carries the lamp and lens is cut at such an angle, that on applying its sloped end to the side of the board carrying the other apparatus, the light is reflected by the polarizing plate in the proper direction without any farther adjustment.

The advantages of this apparatus are, that with a very small specimen, by day or by night, it will exhibit all the phenomena of plane and elliptically polarized light to the greatest angular extent that the eye can receive, and with conveniences of measurement that have never before been given: that thus a macted crystal, or a piece of unannealed glass, &c. can be as it were dissected by successive examination of different small portions: that any accidental roughness or inequality of the crystal produces no sensible effect: and that, by placing the specimen in another position, the macted structure can be exhibited with singular clearness.

In the examination of the phenomena which quartz presents when exposed to circularly polarized light, detailed in my paper before alluded to, I found very great difficulty in consequence of the contraction of the field of view when Fresnel's rhomb is

used with the common polarizing apparatus. In examining the two spirals inwrapping each other, I could see little more than a single line at a time: and it was only by carefully turning the crystal that I could discover the relation of one line to another. It was in fact principally to overcome this difficulty that I devised the apparatus here described: with this I can at once see the folds of the spirals as far as the colours are sensible. I have much satisfaction in seeing that my delineation was quite correct.

On considering my hypothesis relative to the nature of the two rays of quartz, the following method suggested itself as a means of verifying one part of the hypotheses, and as affording a power of measuring the ellipticity of the rays. Suppose (by placing Fresnel's rhomb in a position between 0° and 45° , or between 90° and 135°) elliptically polarized light is made to pass through quartz. Whether this be right-handed or left-handed, there is one direction (*A*) in which one ray of the quartz (suppose for instance the ordinary) is of just the same kind as the incident elliptical light. Consequently that light furnishes no extraordinary ray. Now if we take a direction (*a*) nearer to the axis by the smallest possible angle, and another (*b*) further from the axis by the smallest possible angle, than the direction just mentioned (*A*) the same elliptical light incident in the directions (*a*) and (*b*) will furnish extraordinary rays, but the paths of these extraordinary rays will differ (independently of all other causes) by half the length of a wave. For the elliptical light which is of the same kind as the ordinary ray in (*A*) is more elliptical than the ordinary ray in (*a*), and less so than that in (*b*). And therefore when we separate the elliptical light into an ordinary and an extraordinary ray in (*a*), it is the *defect* of its minor axis which produces the extraordinary ray: when we do the same for (*b*), it is the *excess* of its minor axis which produces the extraordinary ray. The vibration therefore which produces the extraordinary ray in (*a*) being in the positive direction, that which produces the extraordinary ray in (*b*) will be in the negative direction, or *vice versa*. And this amounts to the same as retardation or acceleration by half the length of a wave. It will readily be seen that this is independent of the crystalline separation of the two rays, and is true, however small be the

angle between the directions (*a*) and (*b*,) provided that one be nearer to the axis, and the other further from it, than (*A*.) Now it is well known that the order of the rings depends on the number of lengths of wave which the extraordinary ray is in advance or retardation of the ordinary ray. At this place the advance or retardation is suddenly altered by half a wave. Consequently the order of the rings is suddenly altered by half an order: the rings become faint, and then there is a *saltus* of half an order in the colours. And the direction of the ray where this *saltus* takes place being observed, it gives the direction in which the ordinary ray has the same ellipticity as the incident light, which is known from the position of the rhomb.

By this reasoning I had satisfied myself that the relation between the direction of the ray and its ellipticity could be made evident to the eye. On trying it with the apparatus just described, I found that the appearance was exactly what I expected. On placing the rhomb in position 315° , the spirals are perfect. On turning it forwards, the internal folds break successively, (if the crystal be left-handed) in a line nearly horizontal, and the upper part of each unites itself with the lower part of the fold next beyond it. This continues, the outer folds being broken after the inner ones, till when the rhomb has reached 0° the appearance is that of perfect circles. On turning still forwards, the successive circles break (beginning with the outer ones) in a line nearly vertical; and when the rhomb has reached the position 45° , the spirals are perfect as before, but twisted 90° . Thus at any position of the rhomb intermediate to 0° , 45° , &c. one or more of the inner circles are complete, but distorted, and the exterior circles are changed to two inwrapping spirals. It is plain that the points where the spirals begin are the points where the elliptical light supplies only the ordinary ray, as the colours one quarter of an order within these points, and one quarter of an order beyond them, are the same.

The conclusion, that the inner rings will be circles, and the outer ones two spirals, follows easily from this consideration. The intromitted elliptical light has a smaller minor axis than the ordinary ray (which is the only one that it resembles) in the directions nearest to the axis of the crystal, and therefore it will produce curves analogous to those produced by plane-polarized

light; that is, analogous to circles. But it has a larger minor axis than the ordinary ray, in the directions far from the axis of the crystal, and therefore it will produce curves analogous to those produced by circularly polarized light; that is, analogous to two spirals infolding each other. There is no difficulty in making a more accurate investigation, on the principles described in my former paper: but I have not done it for a reason that will shortly appear.

I have not yet had the opportunity of making measures which are sufficient to point out the law that connects the ellipticity of the rays with the angle that they make with the axis. The following points, however, are made out. One of the rays certainly is right-handed elliptical, and the other certainly left-handed elliptical (or so nearly, that no difference is distinguishable.) The major axis of one is certainly perpendicular to the principal plane of the crystal, and the major axis of the other is certainly in that plane.

In some trials of measuring the ellipticities of the rays, I seem to have arrived at the following conclusion. The proportion of the axis of the ordinary ray is more nearly one of equality than the proportion of the axis of the extraordinary ray. For instance, with a right-handed plate of thickness 0,38 inch, and using a red glass, the first red ring was rendered ambiguous (if I may use that term to denote the state when the ring is broken in such a manner, that it is difficult to say whether the part on one side is most nearly connected with the exterior or interior part on the other side) by supplying an ordinary ray only, with elliptical light, whose

$$\frac{\text{minor axis}}{\text{major axis}} = \tan 17^{\circ}. 15';$$

or by supplying an extraordinary ray only with light, whose

$$\frac{\text{minor axis}}{\text{major axis}} = \tan 16^{\circ}. 2'.$$

With a left-handed plate of thickness 0,16 inch, the first red ring was rendered ambiguous by supplying an ordinary ray only with elliptical light, whose

$$\frac{\text{minor axis}}{\text{major axis}} = \tan 9^{\circ}. 3',$$

or by supplying an extraordinary ray only with light, whose

$$\frac{\text{minor axis}}{\text{major axis}} = \tan 8^{\circ}. 50'.$$

The first result is the mean of 8 measures of each, and the second the mean of 4 measures. The zero points of the rhomb-graduation were determined by observing when the rings of calc spar were not broken. This determination is very accurate; but if it were faulty, the effect of any error in the zero point, as well as of any imperfection in the construction of the rhomb (such as I believe to exist in it) would operate in different ways with right-handed and left-handed plates: and it is on this account that I have given both. No error is to be apprehended from the jogging of the circle carrying the rhomb, as it is provided with four verniers, at 90° apart, all which were read. If this point should be made out, it may be only a consequence of the separation of the rays within the quartz: or it may be another anomaly to be added to the already sufficiently complicated phenomena of quartz. At all events, regarding the perfect equality of the ellipticities as doubtful, I have carried no farther the investigations made on that supposition: though the difference of the ellipticities is so small, that their error would be insensible.

To any one who wishes to proceed with these experiments, the following hints may be of some use.

It is convenient to have a wire carried by the rhomb, parallel or perpendicular to the plane of reflection within the rhomb; and in the place which I have mentioned as the position for a micrometer: as the observer will then see that a line from the center of the rings to the point where the ambiguity takes place, is either parallel or perpendicular to the wire.

The elliptically polarized light is of the same kind (always right-handed) when the position of the rhomb is between 0 and 90° , or between 180° and 270° : and of the same kind (always left-handed) when the position of the rhomb is between 90° and 180° , or between 270° and 360° .

The proportion of the axis is the tangent of the reading of the rhomb position. The major axis is parallel to that edge of the end of the rhomb which makes the greatest angle with the plane of reflection at the polarizing plate.

When the ambiguity takes place near the right or left hand of the center, it is the ordinary ray only which is furnished.

When it takes place nearly above or below the center, it is the extraordinary ray only which is furnished.

The following conjecture may, perhaps, without impropriety, be attached to this paper. It is the suggestion of an explanation of the unequal refrangibility of differently coloured rays.

To account for the difference of refrangibility, we must suppose that the velocity of waves of different lengths is different either in air, or in the refracting medium, or in both. If it were different in air, it would affect the aberration of stars by a quantity that might be sensible; there is no reason to think that this is true. It is probable therefore that the difference is wholly within the refracting medium. Now it is particularly to be remarked, that the difference of velocity does not depend on the magnitude of vibration of each particle, for it is the same, whether the light be feeble or intense, that is, whether the vibration be small or great. Nor does it depend on the relative vibration of two contiguous particles, as that varies in the same proportion as the last, with a variation of the intensity. The only element which, in conjunction with either of these, will define the undulation, is the time of vibration: and it is in fact the time of vibration which distinguishes the different kinds of light. It would seem natural therefore to seek for an explanation of the difference of velocities in something which depends not on space, but on time. Now we have every reason to think that a part of the velocity of sound depends on this circumstance: that from the suddenness of the condensation of the air, the heat evolved by that condensation has not time to escape, and the elasticity is therefore greater than if it had been slowly condensed: that, in fact, the law of elasticity is altered. Now the conjecture which I have to offer is, that perhaps there may be in refracting media something depending on time which alters their elasticity, in the same manner in which heat alters the elasticity of the air: that as in air the elasticity is greater with a quick vibration of particles than it would be if the vibration were exceedingly slow, so also in the refracting media, the elasticity may be greater with a quick vibration, than with one somewhat slower. Or perhaps the contrary effect may follow:

if the vibration be quick, the latent heat (or whatever it is) may not have time to come to the exercise of its influence on the elasticity. In the latter case the elasticity, and consequently the velocity of transmission, would be greatest for the slowest vibrations (that is for the red rays,) and therefore they would be the least refracted. I am not prepared to say whether the general law of superposition of small vibrations would hold on this supposition.

OBSERVATORY,
April 13, 1831.

G. B. AIRY.

ART. IX.—*Descriptions of two Birds, hitherto uncharacterized, belonging to the Genera Crex and Rallus.* By JOHN BLACKWALL, F. L. S., &c. Communicated by the Author.

HAVING been unsuccessful in my endeavours to identify the following birds with any species recorded in those works on ornithology to which I have access, I venture to describe them as new to science.

Order, *Grallatores*, Illiger.
Family, *Rallidæ*, Leach.
Genus, *Crex*, - Bechstein.

C. pygmaea. The bill is black ; the upper part of the head and the back of the neck are black, with a slight tinge of a deep olive colour ; the anterior portion of the back is plain olive-brown ; the lower part of the back, the scapulars, and upper tail-coverts being olivaceous-black, thickly spotted with white ; the upper wing-coverts are blackish-brown, marked with numerous white spots. Quill feathers of the wings blackish-brown ; the first having a row of small white spots on its outer web ; the tertials are conspicuously spotted with white, and the flexure of the wings is yellowish-white. The chin and throat are of a pale cinereous hue : sides of the head and neck, breast, and the anterior part of the abdomen, of a deep ashy-lead colour ; lower part of the abdomen cinereous-black, transversely banded with white ; tail and under tail-coverts deep olivaceous black, the latter dashed with yellowish-white. The legs are pale brown.

Length, from the point of the bill to the extremity of the tail, $4\frac{7}{10}$ inches; to the extremity of the middle toe, 6; wings from the carpal joint to the tip of the third quill feather, $2\frac{5}{8}$; tail $\frac{1}{2}$; bill from the point to the gape, $1\frac{6}{10}$; tarsi, $\frac{3}{4}$; middle toe, including the claw, $1\frac{9}{10}$; hind toe, including the claw, $1\frac{5}{10}$; tibiae, bare of feathers above the knee, $\frac{1}{4}$.

This minute Crake, which is a North American species, was sent to this country from Philadelphia.

Genus, *Rallus*, Auctorum.

R. bicolor. The bill is strong, and of a yellowish-green colour. The upper part and sides of the head are deep cinereous; the occiput and back of the neck olive-brown; back, scapulars, and upper tail-coverts olive-brown, with a greenish tint, which is brightest on the anterior portion of the back; the upper wing-coverts are olive-brown; the quill feathers of the wings olivaceous-black. The chin and throat are pale cinereous; the lower part and sides of the neck, the breast, and anterior region of the abdomen are of a bluish-slate colour; thighs and lower part of the abdomen dull olive-brown, tinged with cinereous; tail, and under tail-coverts, black, with a slight mixture of a deep olive colour. The legs are pale yellowish-brown.

Length from the point of the bill to the extremity of the tail, $12\frac{1}{4}$ inches; to the extremity of the middle toe, $15\frac{3}{4}$; wings, from the carpal joint to the termination of the third quill feather, $5\frac{3}{10}$; tail $2\frac{9}{10}$; bill, from the point to the gape, $2\frac{1}{10}$; tarsi $1\frac{9}{10}$; middle toe, including the claw $2\frac{7}{10}$; hind-toe, including the claw, $\frac{1}{2}$; tibiae, bare of feathers above the knee, $\frac{4}{10}$.

The native place of this bird is supposed to be Brazil, as it was found in a collection of zoological subjects formed in that empire.

The specimens from which the foregoing descriptions were taken are in the extensive and valuable museum belonging to Mr Robert Wood of Manchester, who obligingly submitted them to my inspection. Mr Wood has also in his possession a fine specimen of that rare bird, the *Scolopax gigantea* of M. Temminck.

ART. X.—On *Plumbo-Calcite*, a Carbonate of Lime and Lead.

By JAMES F. W. JOHNSTON, A. M. &c. &c. Communicated by the Author.

Among the rubbish of one of the old workings at Wanlockhead, there are found considerable quantities of what appears to be carbonate of lime, beautifully crystallized in the primary rhomboid. My attention was first directed to this mineral by the peculiar pearly lustre which some of its faces present, and the slight rounding which its surfaces occasionally exhibited. It occurs also massive and opaque, the transparent crystals being generally formed in cavities either single or in groups.

From a hot solution in muriatic acid, I was surprised to find that, on cooling, beautiful white prismatic crystals were deposited, which before the blowpipe on charcoal gave globules of metallic lead.

Heated in a platinum crucible, or in an open tube, the mineral decrepitates, and after a considerable time assumes a brownish or pale reddish tint, which may arise from the decomposition and higher oxidation of the carbonate of lead. Before the blowpipe on charcoal it gives with soda a white enamel, but no globules of lead that I have been able to discover. A small fragment, however, dissolved in muriatic or nitric acid, gives a white precipitate with caustic ammonia, becoming black by the addition of hydrosulphuret of ammonia, and giving before the blowpipe a globule of metallic lead.

It is scratched by Iceland spar, and has at 60° Fahr. a specific gravity of 2.824 in an imperfectly crystallized specimen.

47.57 grains dissolved in dilute muriatic acid, the gas caused to pass over chloride of calcium, and the solution heated till the globules of gas had nearly ceased to be evolved, had lost 19.655 grains = 41.318 per cent. of carbonic acid.

The lead thrown down by sulphuretted hydrogen and converted afterwards into sulphate, and the lime precipitated by oxalate of ammonia, gave the constituents in the following proportions.

Car. of lime 43.86 = 92.2 = 40.20 Carbonic acid = 80 atoms.

Car. of lead 3.71 = 7.8 = 1.36 ————— = 1.014 atoms.

Iron a trace

47.57 100. 41.56

On making out this composition, I remitted to Dr Brewster the only specimen I could then find, and which was only imperfectly crystallized, with the view of ascertaining what difference in the angles of the rhomboid was caused by this admixture of carbonate of lead. He informed me that the faces were all rounded, and that the mean of six measurements gave for the obtuse angle $104^{\circ}.53\frac{1}{2}'$ which was too near $105^{\circ}.5'$ to enable him, from such imperfect crystals, to say that any difference really existed.

It was rendered probable by this result that the carbonates of lead and lime were isomorphous; and it occurred to me that it had already been shown by Mitscherlich that carbonate of lead was isomorphous with Arragonite. To this Rose (Poggendorf's *An.* vol. ix. p. 187,) has added, that the phosphate of lime (apatite) is isomorphous with the phosphates and arseniates of lead, Heeren, that the hyposulphites of lead, lime, and strontian, (Gmelin's *Handbuch*, i. p. 1082,) and Levy, that the tungstates of lead and lime are isomorphous; and, lastly, Kersten (Schweigger's *Neues Jarbuch*, ii. p. 21,) has deduced the same result respecting the two bases from an analysis of seven varieties of phosphate of lead, in all of which he found a variable portion of phosphate of lime taking the place of the phosphate of lead without altering the crystalline form.

It is extremely interesting, then, as confirming all these previous results, to find in the mineral above described the carbonate of lead taking the place of the carbonate of lime without altering the form of the crystal, and it renders stronger the conclusion, that the oxides of lead and calcium are isomorphous. But bases, it is obvious, *though isomorphous, cannot replace one another unless their combining proportions or crystalline atoms be also equal in volume.* For supposing an indefinite number of atoms of the same shape, but of a different size, to be enveloped in a crystal of any substance, spaces would necessarily be left, and the crystal would not be homogeneous. The doctrine of replacement, therefore, implies two conditions,—isomorphism and equality of volume. In any pure crystallized mineral, then, where such replacement occurs, the specific gravity must be a mean between that of the replacing and of the

replaced body, and thus by a careful estimation of the specific gravity we *should* have always a check upon our analytical results. But there are so many sources of error in estimating specific gravities that we shall probably never be able to do more by this method than deduce rough approximations.

Beudant has shown that the specific gravity of small crystals is greater than that of larger, and that when broken into fragments, or reduced to powder, that of the large crystals is increased; while Breithaupt has found that fragments of the same mineral species, (calc spar,) vary in specific gravity between 1 and 2 per cent. with the crystalline form*. These, with the variations of thermometers, and the employment of impure specimens, are some of the causes of the great discrepancy we find among authors in regard to specific gravities. Thus, Phillips gives for carbonate of lime 2.717; Mohs, 2.721; and Beudant, 2.723; while for carbonate of lead we have the three values 6.72, 6.465, and 6.729.

If we take the specific gravities of Mohs, and test the above analysis, we have $(2.721 \times 30 + 6.465) \div 31 = 2.84$ for the specific gravity of a compound containing the carbonates of lime and lead in the atomic proportions of 30 : 1, while the specific gravity by experiment is 2.824.

Though I have found this ratio between the carbonates in two fragments of the same mass, yet we can hardly consider it as a constant ratio forming a definite mineral species. In other specimens the lead may increase or decrease till the crystals become wholly lead or wholly lime; and I propose the name *plumbo-calcite* as a short way of designating all such varieties of the mineral as may contain lead in any proportion. That isomorphous substances may replace each other in different proportions in different parts even of the same crystal has been beautifully shown by Mosander, in his interesting analyses of titanite iron.† He found this to be a compound of proto-titanate of iron with peroxide of iron, and that these two, the proto-titanate and the peroxide, were isomorphous, and so intermingled, that one part of a crystal contained so much iron as strongly to attract the magnet, while another part scarcely

* Schweigger's *Neues Jahrbuch*, vol. ii. p. 125.

† Berzelius *Arsberättelse*, 1830, p. 171.

affected it. In regard to all such minerals, it is obvious that no two analyses can agree.

It is probable that oxide of lead may exist in many primary carbonates of lime, in which, the form remaining unaltered, it has hitherto escaped observation.

One new and important fact deducible from the composition of this mineral must not be neglected. The observation of Mitscherlich, that Arragonite and carbonate of lead are isomorphous, showed only that lime and lead in certain circumstances are isomorphous. In the rhomboidal carbonate of lime, the atoms arrange themselves so as to produce a form belonging to an entirely different system from that of the Arragonite. The only legitimate inference, therefore, was, that lime has two crystalline forms or arrangements, in one of which it is isomorphous with lead. The other results of Rose, Heeren, and Levy only confirmed this inference; they carried it no farther, and threw no new light upon it. But in the plumbo-calcsites we have a phenomenon exactly the converse of that observed by Mitscherlich. We have carbonate of lead, usually isomorphous with Arragonite, assuming the form of the rhomboidal carbonate of lime, showing that lead also has, or is capable of assuming, two different crystalline forms. Lime, therefore, no longer presents an anomaly. It may probably only prove one of a large family; for there is no reason why those bodies which have been found isomorphous with lead and with lime respectively, should not like these two bases be mutually isomorphous. This view throws open a wide field for crystallographic research. Where substances are found in two irreconcilable forms, they may properly be called *bimorphous*, to which class belong sulphur, lime, and protoxide of lead; while those which assume two forms respectively isomorphous, would be accurately named *isobimorphous*. To this class of isobimorphous bodies belong lime and protoxide of lead. Others, it is probable, will soon be added.

There being no anomaly, then, in lime entering into two crystalline forms belonging to different systems, we need hesitate no longer in considering Arragonite when pure as carbonate of lime alone, and the strontia an isomorphous body where it occurs as an impurity.

M. Germain Barruel published some time ago*, the analysis of a mineral crystallized in the primary rhomboid of carbonate of lime possessing double refraction, and having a specific gravity of 2.921, which he found to consist of carbonate of lime, 70; carbonate of soda, 14; water, 9.7; oxide of iron, 1; gangue, 5. Of this analysis Berzelius, in his *Arsberättelse*, (1830, p. 163,) says that it is probably a mistake, and that the soda has very likely got in by some mistake with the re-agents.

PORTOBELLO, 18th Nov. 1831.

ART. XI.—*On the principle of Illumination for Microscopic Objects.* By DAVID BREWSTER, LL. D. F. R. S.

IN the numerous observations which I have had occasion to make with microscopes of almost all kinds, I have been very much struck with the difference of effect which was produced by different methods of illumination. To throw light upon a microscopic object sufficient for the purposes of vision, and to regulate the intensity of the light by employing a larger or smaller section of the convergent beam reflected by the illuminating mirror, or refracted by the illuminating lens, has been the general process of observers.

I had long ago found that a bright pencil of light issuing from the smallest possible point was highly favourable for microscopic vision, and I have explained the reason of this in the article MICROSCOPE in the EDINBURGH ENCYCLOPEDIA. As it is difficult, however, in ordinary cases, to obtain such a brilliant centre of illumination without using mirrors or lenses, it becomes necessary to determine the proper method of concentrating the light and throwing it upon the object.

In his ingenious paper on a Microscopic Doublet, published in this *Journal*, No. ii. p. 323, the late Dr Wollaston has given a new method of illuminating objects which has been highly commended. He places a circular perforation about three-tenths of an inch in diameter, *between* the illuminating flame and the illuminating lens, which is about three quarters of an

* *An. de Chim.* xlii. p. 312.

inch focus, and he directs that a distinct image of this perforation be thrown upon the object, or be formed "in the same plane as the object which is to be examined." Dr Wollaston has not explained the reason why he has adopted this method ; but we are persuaded that he committed an oversight in supposing, as he seems to have done, that the light which he uses radiates from the perforation whose image he throws upon the object. Let us suppose that a minute microscopic object is illuminated by being placed in the centre of the small circular image of the circular perforation, which he says is about the 20th of an inch in diameter ; then of what avail is it that the margin of this circular image is distinct,—for the eye need not even see this distinct margin. If the light is supposed to radiate from the plane of the real circular aperture, then the light will have its points of convergence or foci in the plane of the circular image, and in this case the distinctness of the margin would be the proof that the rays converged to points in its plane : But as the light does not so radiate, we are at a loss to discover the object which the distinguished author had in view by the arrangement which he recommends.

Dr Wollaston's method of illumination is, in short, entirely independent of the distance of the lamp, or candle, or luminary, from which the light proceeds, and it is obvious that the rays which fall upon the object must be in different states of convergency when they radiate from a near or from a distant lamp.

In order to explain still farther what we conceive to be the fault of this method of illumination, let us suppose the object under examination to be a small spherical seed the 10,000th part of an inch in diameter, then, if it is illuminated by Dr Wollaston's method, where the radiant point of illumination is *at a distance* from the anterior conjugate focus of the illuminating lens, or from the circular perforation, the shadow of the sphere and the image of the sphere cannot possibly be coincident, or of the same apparent magnitude, and in general the shadow of one part of the microscopic object will fall upon the image of another part, and occasion those imperfections of illumination and of vision which prevent the distinct separation of minute parts, and the clear vision of delicate structures.

The true principle, therefore, of the illumination of micro-

scopic objects is to obtain the most perfect convergence of the illuminating rays upon the points of the object under observation,—an effect which can only be produced by placing the illuminating flame close to the circular perforation, or, what is better still, to form by a second lens a luminous image of the flame in the plane of the perforation. The light which passes through the perforation will, therefore, have its points of divergence coincident with the margin of the perforation, and the distinct image of the perforation formed by the lens in the plane of the object, will be a proof that the rays have their foci, and consequently their new points of divergence in that plane.

I have no hesitation in saying that the apparatus for illumination requires to be as perfect as the apparatus for vision; and on this account, I would recommend that the illuminating lens should be perfectly free of chromatic and spherical aberration, and that the greatest care be taken to exclude all extraneous light, both from the object and from the eye of the observer.

When I had the satisfaction of examining at York the beautiful reflecting microscope executed by Mr Potter, and described in a preceding article, (see page 61,) and which was equally remarkable for the perfection of its workmanship, and the fine vision which it gave of very difficult objects, I explained to him the general principle of illumination which has now been mentioned. Upon applying this principle, he found, as he writes me, a much greater improvement from it than he expected; and he observes in the article already quoted, “that it was only after adjusting the illuminating lens of his microscope very carefully, that he saw with it the complete structure of a regularly woven net,” in the blue band of the *Chubiona atrox*.

ALLERLY, Nov. 29th 1831.

ART. XII.—*On the Progressive Expansion and Maximum Density of Water.* By S. STAMPFER, Professor of Practical Geometry in the Imperial Polytechnic Institution of Vienna.*

IN a long and very elaborate memoir, having chiefly in view to determine the absolute weight of water contained in the Austrian standard measures, Professor Stampfer has obtained results differing considerably from the results of former experimenters.

The maximum density he found to be at $3.75\text{ C} = 38.75\text{ F.}$, and the expansions and densities above and below that point are contained in the following table.

Temperature.	Density.	Differences.	Volume.	Differences.
— 3 C.	0,999627		1,000373	
2	0,999731	104	1,000269	104
1	0,999818	87	1,000182	87
0	0,999887	69	1,000113	69
+ 1	0,999939	52	1,000061	52
2	0,999975	36	1,000025	36
3	0,999995	20	1,000005	20
3,75	1,000000	5	1,000000	5
4	0,999999	1	1,000001	1
5	0,999988	11	1,000012	11
6	0,999962	26	1,000038	26
7	0,999921	41	1,000079	41
8	0,999865	56	1,000135	56
9	0,999795	70	1,000205	70
10	0,999711	84	1,000289	84
11	0,999613	98	1,000387	98
12	0,999503	110	1,000497	110
13	0,999380	123	1,000620	123
14	0,999244	136	1,000757	137
15	0,999095	149	1,000906	149
16	0,998935	160	1,001066	160
17	0,998763	172	1,001239	173
18	0,998580	183	1,001422	183
19	0,998386	194	1,001617	195

* Poggendorf's *An.* xxi. p. 75.

Prof. Stampfer on the Expansion and Density of Water. 87

20	0,998180	206	1,001822	205
21	0,997965	215	1,002089	217
22	0,997740	225	1,002265	226
23	0,997504	236	1,002502	237
24	0,997259	245	1,002749	247
25	0,997003	256	1,003005	256
26	0,996740	263	1,003271	266
27	0,996468	272	1,003545	274
28	0,996187	281	1,003828	283
29	0,995898	289	1,004119	291
30	0,995601	297	1,004418	299
31	0,995296	305	1,004726	308
+32 C	0,994984	312	1,005041	315
33	0,994665	319	1,005363	322
34	0,994338	327	1,005694	331
35	0,994004	334	1,006032	338
36	0,993665	339	1,006375	343
37	0,993320	345	1,006725	350
38	0,992968	352	1,007081	356
39	0,992611	357	1,007444	363
40	0,992247	364	1,007813	369

Professor Muncke, in his *Physical Dictionary*, has inserted a series of results on the same subject, in which he found the maximum density at $3^{\circ}.78 = 38.804^{\circ}$ F. The following table shows the differences between Stampfer's results and those of Hällström and Muncke, and the table of Biot.

Temp.	Stampfer.	Biot.	Hällström.	Muncke.
0° C.	0,999887	+0,000038	+0,000005	+0,000002
5	0,999988	—0,000003	+0,000007	+0,000001
10	0,999711	+0,000019	+0,000072	+0,000011
15	0,999095	+0,000076	+0,000170	+0,000034
20	0,998181	+0,000138	+0,000272	+0,000067
25	0,997004	+0,000188	+0,000355	+0,000107
30	0,995601	+0,000193	+0,000391	+0,000145
35	0,994004	+0,000137		+0,000174

ART. XIII.—*On the Guano, the Ornithocoprus of Peru.* By
J. C. POGGENDORF.*

PROFESSOR BUCKLAND, in his account of "the discovery of fossil fæces in the Lias at Lyme Regis, and in other formations,"† has stated, that, in certain parts of England, there exist formations, miles in length and of considerable thickness, in which the calcareous remains of the fæces of antediluvian animals constitute a fourth part of the whole. This fact is certainly astonishing, but it loses all improbability when we call to mind the Guano of Peru. The excremental nature of this substance has been clearly established by the analyses of Klaproth, ‡ Fourcroy, and Vauquelin, and yet it forms beds on the coast of Peru of such extent that we can hardly persuade ourselves to consider it—what every circumstance shows that it must be—the dung of sea-fowl formerly living on the spot.

The name *Huanu*, (which Europeans have changed into Guano), in the language of the Incas, means, according to Humboldt, *dung* employed for manure. The verb to manure is *huanunchani*. The aborigines of Peru universally consider the guano to be the dung of fowls; it is only some of the Spaniards who question it. The guano is found only on the coast and in the islands and cliffs between the 13th and 21st degrees of south latitude, and it forms here beds 50 or 60 feet deep, which are wrought like mines of iron ochre. Beyond these latitudes, either north or south, it is not found, although there scars, flamingos, cormorants and cranes seem equally abundant. Near the town of Arica the little *Isla di Guano* spreads so dreadful a stench, that, according to Feuillé, vessels sailing along the coast keep as far from the town as possible. In Arica are built immense magazines along the shore, in which the guano is stored up.

When we consider that, as far back as the 12th and 13th centuries at least, the custom has prevailed of manuring with

* From Poggendorf's *Annal.* xxi. p. 604.

† *Geolog. Trans.* S. S. iii. p. 223.

‡ Klaproth, *Beitrag*, iv. p. 299, found the guano to consist of 16 uric acid containing ammonia, 10 phosphate of lime, 12.75 oxalate of lime, 4 silica, 0.5 common salt, 28 sandy mixture, 28.75 water and combustible matter. The French chemists found 25 per cent. uric acid.

the guano,—that many millions of cubic feet have been spread over the sandy parts of Peru, where the possibility of cultivation along the sea coast is solely due to the presence of this valuable material,—that the guano can never be reproduced in equal quantity,—nay, that experience has proved that the birds upon an island appear in several years to produce only one or two ship loads,—we cannot but be astonished at the many centuries which must have elapsed, and the multitudes of sea-birds which must have come and gone during the raising of a single deposit of guano. Yet that these masses owe their formation solely to the sea birds is confirmed by the observation of Frezier, that feathers have been found in them at great depths.

During the reign of the Incas the guano was considered of high importance to the national economy. It was a capital crime to kill the young birds on the Guano Islands. Each island had its overseer, and each supplied certain provinces. From *Arica* to *Chaucay*, an extent of 200 leagues, the country was manured with guano alone.* Under such prudent management we can understand how the guano should increase so considerably. All these excellent regulations have long been done away.

These remarks are confirmed by Mariano de Rivero in a memoir in the Spanish language, with which I am acquainted only by a short extract contained in Ferrussac's *Bulletin*, sect. I. vol. xi. p. 84. He says "the Spaniards permitted the wise regulations of the Incas, by which the continuance of this valuable manure was insured, to pass into forgetfulness. The modern Peruvians have become sensible of their fault, and see with regret the time fast approaching when the guano will no longer be found in sufficient quantity to meet the demands of agriculture."

The discovery of new beds of the brown guano, which is of very old formation, is becoming daily less frequent, while the reproduction of white guano is rapidly diminishing, since the freedom of trade has brought so many ships to the coast disturbing and driving away the birds to other parts. Nevertheless, the yearly consumption of white and brown guano is at

* About Villacori the ancient Peruvians used to manure their land with fish cast ashore by the sea.

present from six to eight thousand tons, and the cost at the sea ports from which it is conveyed inland from 180 to 200 thousand heavy piastres.

To the guano Dr Buckland has given the name of *Ornithocopus*.

ART. XIV.—*On a Restoration of a Proposition in Pappus.*

By the Rev. CHARLES FREDERICK BARNWELL. Communicated by the Author.

IN the introduction to the 42d proposition of the 4th book of Pappus, the author states that he had resolved the solid inclination assumed by Archimedes in his *Treatise on Spirals*, in order to enable the reader of that work to proceed in this part of it without hesitation. He then gives, in propositions 42 and 43, two local theorems, employed in the resolution of the problem; and with the latter of these the 4th book, in Commandine's translation, ends abruptly, without the least trace of the application of these theorems to their purpose.

In the Greek MSS., however, we find the earlier part of the 44th proposition, which contains the analysis of the problem in question: but in so imperfect a state, * that we are led to conclude the translator had not been able to satisfy himself as to its restoration, or at least that he had left nothing on the subject (the work having been printed after his death) in a sufficiently perfect state for insertion in its place.

In the MS. of Pappus, (called by Dr Simson Codex Bulialdi,) in the Advocates' Library at Edinburgh, is a separate leaf, containing some emendations of this fragment by a recent hand, probably Dr James Moor, to whom that MS. formerly belonged.† It does not, however, appear that he had carried the restoration of the proposition any further: nor does anything more than a very imperfect sketch of the diagram occur in that leaf, though it seems clear, from the nature of the emendations, that the writer had arrived at the correct analysis of the problem. Having been, some years since, favoured with a copy of this paper by my late most respected friend, the author

* See Dr Trail's *Memoir of the Life and Writings of Dr Simson*, p. 148.

† Trail's *Life of Simson*, p. 177.

of the excellent memoir to which I have already referred, I succeeded in restoring the proposition, as follows :

A circle $ABGC$, and a straight line BC placed in it, being given in position, and a point A in the circumference being also given, to place between the straight line BC and the circumference BGC , a straight line equal to a given straight line, and tending to the point A . Plate I. Fig. 3.

Analysis.—Suppose it done, and let GF , passing, when produced, through A , be the line required. Draw FE perpendicular to BC , and equal to AF . Then, by proposition 42, the point E will be in a hyperbola given in position. Again, since the rectangle $EF, FG = AF, FG = BF, FC$, and FG is given, the point E , by proposition 43, will be in a parabola given in position. Hence the point E is given : and, since the straight line BC is given in position, the point F and the position of the straight line AFG are also given.

It is obvious, from the nature of the problem, that the length of the given line must not exceed a certain limit : a determination which is also indicated in the above analysis, it being necessary to the composition that the hyperbola and parabola should meet each other.

Composition.—Let the given straight line be H : draw AKM perpendicular to BC , and meeting it in K : take $KD = AK$, and on AD , as the transverse axis, describe the equilateral hyperbola DEL . Again, with H as the parameter, and BC as a double ordinate, describe the parabola BEC . Then, if this parabola does not meet the hyperbola, the problem will be impossible : but if otherwise, let E be a point of concurrence, and from E draw EF perpendicular to BC : join AF , and produce it to meet the circumference in G : then FG will be equal to the given straight line H . For, by proposition 43, $EFH = BF, FC = AF, FG$. But by proposition 42, $EF = AF$: consequently $FG = H$.

Determination.—Let the parabola be in the first instance supposed to touch the hyperbola : then the problem will have a single solution, and the latus rectum of this parabola will be the greatest possible value of the given straight line H . For, if H be supposed greater than this line, since the ordinate BC is common, the abscissa of the parabola, whose latus rectum

is H , will be less than that of the touching parabola: the former parabola will therefore fall entirely within the latter, and consequently, will not meet the hyperbola, and the problem will be impossible. But if H be supposed less than the latus rectum of the touching parabola, then, for a similar reason, the parabola, whose latus rectum is H , will fall entirely without the touching parabola, and will therefore cut the hyperbola in two points, and the problem will have a double solution, as in the figure.

The assumption in proposition 8 and 9 of Archimedes on Spirals is a particular case of this problem, where the given line $\equiv KM$. The parabola will, in this case, (since $MK, KD \equiv MK, KA = BK, KC$) pass through the point D : and, since BK is supposed unequal to KC , the tangent to the parabola at D will make oblique angles with AD , while that to the hyperbola at the same point will be perpendicular to AD : consequently the parabola will cut the hyperbola, and a second line will be found, (KM itself being one,) answering the conditions of the problem.

In propositions 6 and 7 of the same treatise, there is a similar assumption, except that the line of given length is supposed to be intercepted *without* the circle, that is, between the circumference and BC *produced*. This is, in fact, no more than another case of the above problem: and the same solution will (*mutatis mutandis*) apply to both, if the 43d proposition of Pappus be extended to the case where the point C (see the fig. in Commandine,) is in AB *produced*, and consequently, EF and CD are on opposite sides of AB .—The figure will now be of the kind shown in Plate I. Fig. 4. And, since the parabola (as may be without difficulty shown,) will always meet the hyperbola in two points, this case of the problem will have no determination, and the solution will always be twofold, as, indeed, is immediately evident from the nature of the case.

At the end of the fragment in the Greek MSS. follows a remark, that this inclination had been employed by Archimedes for the purpose of exhibiting a straight line equal to the circumference of a circle, * and that certain persons had animadverted on this use of it, on the ground that the same purpose might have been effected by a plane construction, with the

* *Treatise on Spirals*, prop. 18th, in which the 7th is employed.

help of the previously demonstrated theorems respecting the spiral. A similar observation, applied also to the 58th Proposition of the 5th Book of *Apollonius's Conic Sections*, occurs in another passage of Pappus, Book 4th, and is given in the original Greek in pp. 185, et seq. of Trail's *Life of Simson*.

From these remarks it appears, that, in the time of Archimedes and Apollonius, either the rule, which prohibits the use of conic sections in the resolution of plane problems, had not been strictly laid down, or that all problems into which a conic section or superior curve entered, even as one of the data, were then considered as of the solid or linear order. But that the later geometers, to whom Pappus refers, had adopted the maxim afterwards laid down by Sir Isaac Newton,* that such problems of this latter description as are capable of being resolved by the help of the circle only, *in addition to the data*, are truly plane, and, as such, fall under the general rule. Some excellent observations connected with this subject may be found in the *Life of Dr Simson*, pp. 136–141. See also p. 29, note.*

ART. XV.—*Notice on some New Modifications in the Phenomena of Newton's Rings.* By G. B. AIRY, M. A. Plumian Professor of Astronomy, Cambridge.

AT a late meeting of the Cambridge Philosophical Society, a paper was read by Professor Airy, on some new modifications in the phenomena of Newton's rings. It was observed, that in the question concerning the explanation of these rings by Newton's theory of fits of easy transmission and reflexion, and by the rival theory of undulations, the leading difference is, that, on the latter supposition, the portions of light reflected from both of the surfaces containing the thin plate of air are concerned, and the rings are produced by the interference of these portions: while in Newton's theory the light reflected at the second surface alone produces the colours which are observed. Professor Airy remarked, that, in order to decide between these two theories, it might have occurred to the disputants

* *Arithmetica Universalis* (*Æquationum constructio linearis.*)

that an *experimentum crucis* would be found, if we could procure a kind of light which is capable of reflexion from one of these surfaces and not from the other. So long as both the surfaces are of glass, this elimination of one of the portions of light is not possible ; but if a lens be laid in a surface of *metal*, and the light incident be polarized light at the polarizing angle, the rings ought to disappear if the doctrine of undulations be true, while on the other supposition they would still be seen. By these and similar considerations, Professor Airy was led to predict certain peculiarities in the phenomena of the rings of thin plates of air contained between glass and metal ; one of which peculiarities is, that, by varying the angle of incidence, the rings would first vanish, and then reappear with complementary colours. The phenomena thus predicted were in all respects confirmed by an appeal to experiments, and the paper read contained a description of these, and an exposition of the mathematical calculations by which their details are deduced from the doctrine of undulations. Professor Airy stated also, that his reasonings lead him to predict further, that, if the Newtonian rings are thus formed by a plate of air contained between two substances, one of high and one of low refractive power, they will, in the course of the changes of the angle of incidence, vanish at a certain angle, then assume a complementary character, vanish again at another certain angle, and then reassume their original colours : these two critical angles being the polarizing angles of the two substances. The author stated that he had not yet had an opportunity of procuring substances by which he could verify these predictions.

ART. XVI.—*Notice respecting a Vitrified Fort at Carradale in Argyleshire.* By JAMES D. FORBES, F. R. S. E., F G. S. Member of the Royal Geographical Society of London.

IN the study of effects produced by human agency in remote times, we are no less called upon to exercise the patiently inductive method of arriving at the final cause, than in the examination of natural phenomena. Now the existence of that

class of objects generally designated Vitrified Forts, is accompanied by such phenomena as render the inquiry into their history and mode of construction interesting and important beyond the mere historical announcement in which the discovery of their object would be contained: and the nature of these has been so varied, and in some instances so apparently opposed, as to render the combination of as many authentic data as possible desirable, especially as the evidence of design or mode of operation is often by no means decisive and important in proportion to the magnitude or state of preservation of the fort.

Under this impression, I propose to detail a few particulars of one of these structures, of which, though the existence has been known, I believe that no account has yet been communicated to the scientific world. I do so also under the belief, that, from the number and variety of vitrified forts or sites hitherto described, and the variety of their features, the problem of which they form the data remains an indeterminate one admitting of several solutions, and that, therefore, every new fact tending to limit the field of inquiry must be considered an important aid in such an analytical procedure.

The vitrified fort of Carradale is situated at the extremity of a promontory in the bay of that name in the rather remote district of Cantire in Argyleshire. As the local position is a matter of some importance in the deduction of ulterior facts, I may observe that it stands on a peninsula which is, in fact, detached from the land at the time of high water. The peninsula is rocky, abrupt on all sides towards the sea, and has a flat top of an elongated form. Situated on a deeply indented coast, and forming only one promontory among many, both more extended and loftier, its horizon towards the north is completely cut off, but to the south and east there is a full view of the coast of Argyleshire as far as Campbelltown Bay, of the rock of Ailsa, the yet more distant coast of Ayrshire, and the Island of Arran comparatively in the fore-ground.

The fort, or rather (for it hardly deserves that name,) inclosure, consists of a continuous and well compacted wall adapted to the ovoidal form of the eminence. The quantity of materials which it contains appears to me extremely small, and as

there has been no temptation for their removal, so common an occurrence in other cases, we are necessarily led to the conclusion, that the walls must have been extremely inconsiderable in height or strength, and, therefore, could hardly have been intended as a place either of defence, or calculated to afford much shelter. An idea of the area it includes may be formed when I mention that its circumference is about 150 yards, its greater and less diameter 60 and 25 yards respectively.

As it is not my intention at present to offer any general remarks upon the true theory of these singular works of art, I shall confine myself to a brief statement of such facts as appear to bear upon existing hypotheses, and especially that of beacon fires, which has recently been supported with much force and plausibility by my friend Dr Hibbert.

Upon the whole, this specimen will perhaps be found to offer as considerable objections to that opinion, considered as a universal explanation, as any of which I am aware. Its general character is considerably different from that of those which have usually been described. In combating Mr Williams's theory of the origin of vitrified forts, Dr Hibbert observes, that "it would present to view a pile of stony materials with sides which would be vertical or nearly so; and it would inclose the summit of a hill after the manner of a modern park wall. But this is not the character of a vitrified fort. The structure of the least dilapidated example is much more rude, being rather after the model of an extinct or filled up volcanic crater. Upon the summit of an insulated hill, an incredible accumulation of loose stones has been made to rise to the greatest height round its circumference, and to gradually thin off towards the centre of the inclosed site." Applicable as the last description may be to the generality of vitrified forts, the first will be seen to apply much more accurately to the case before us, if I have at all succeeded in explaining its general features. No description can be more accurate than that "it incloses the summit of a hill after the manner of a modern park wall." It is also very striking, that, whilst the vitrification is more than usually perfect and continuous of the wall itself, the inclosed area, though abounding in exposed rock of precisely the same nature with the material of the inclosure, presents no marks what-

ever of the action of fire, which could not have failed to be the case had the summit of the hill been employed as an immense beacon with merely a parapet round for the retention of the fuel. It is also remarkable that the vitrification appears to be as perfect on the exterior as within.

These somewhat perplexing facts for the beacon hypothesis are not lessened in force by the consideration of the rather inconspicuous situation of the structure. Had it been placed a very little farther to the north, an extensive view would have been attained in that direction, without any sacrifice of its present range of the horizon, but rather the reverse. It might then have been connected with other stations towards the centre of the country ; perhaps with the vitrified fort in the Kyles of Bute. At present, though it is not ascertained, I believe that there are no such stations in view from the Bay of Carradale.

The hypothesis of being intended as a place of defence is also put to a severe test by this structure. There is no probability that the wall ever rose many feet from the ground ; and it is commanded by an eminence immediately to the northward. Nevertheless it strongly conveys the impression, that the vitrification was the result of design, and the wall is in general sufficiently removed from the precipice to have admitted of the application of heat externally, which, from the uniformity of the effect, must doubtless have been attended to.

Perhaps we may finally be compelled to admit that the problem admits of several real solutions, and that every specimen of vitrification may not owe its origin to a single cause. But I forbear to speculate upon a subject which I may perhaps resume when a more extensive examination of the vitrified sites of Scotland, and a farther consideration of the subject, may enable me to form a more decided opinion.

Before concluding, I mean to offer a remark or two upon the state of the vitrified materials. I have already observed that we may draw some important hints for the physical sciences from the phenomena of these artificial applications of heat, and especially in reference to geology,—a connection which has not been overlooked by Dr Hibbert, and which seems to have

excited the particular attention of M. Von Leonhard of Heidelberg. Not merely are the processes of subterranean heat now going on under the form of volcanic eruption illustrated by it, but those phenomena, remoter as to date, of the nature of which we can only judge by the striking concomitant effects left imperishably registered in the crust of the globe, are thereby illustrated. That spirit of generalization which first connected the phenomena of the trap rocks with the productions of modern volcanoes, requires only an extension to derive the most important and convincing analogies from the furnace, the lime-kiln, and the vitrified fort. In these we may observe concretionary separations going forward in a manner perfectly similar in result to what we see in the mineral kingdom. We may trace on the small scale the crystallization of simple minerals; the contortions of strata; the production of prismatic structure; and whilst we remain in almost total ignorance of the nature of molecular action, we reach a step nearer the truth by referring to a common origin, effects of which we can never see the direct production, and those which admit of repetition under factitious circumstances.

The geological position of the different vitrified forts permits us to study with considerable facility the action of heat upon various rocks. That of Carradale is situated in a mica-slate district, where that rock occurs almost alone, and remarkably well developed. It is of the harder species, in which quartzose particles are much intermixed with the laminæ of mica, and besides forming occasional pure nodules and veins, give a great degree of hardness to the rock itself. Hence it is more than usually refractory when exposed to heat, and is seldom fusible unless when the layers of mica are sufficiently separated to admit of a distinct reduction, which gives the rock a very peculiar appearance. In other cases the whole substance is merely reddened from the influence of heat. Where the micaceous particles are abundant, and the heat has been sufficiently intense, a complete soldering of the fragments may have been produced from the fusion of these alone. It appears to me, however, (and this is not the only instance in which I have observed it,) that the slag, which often accumulates in large quantities and fills up vacuities, could not have been produced

from this source. It often appears as if fusible at a lower temperature, and assumes the appearance of the beautiful pitch-stone of the neighbouring island of Arran. From a chemical examination of these products, I should expect some rather important general facts.

It happens, that, in the vicinity of this vitrified fort, there exists an interesting illustration of the effects of natural heat upon the identical species of rock which has here been exposed to artificial combustion. A vein of trap at Portree, traversing the mica-slate, has left the most striking marks of the nature of the influence which it exerted upon the surrounding strata; and I regret, that the very short time I had to spare at this interesting spot permitted me only to ascertain some general facts, and bring away a few of the first specimens I could obtain in illustration of a true specimen of natural vitrification. The vein or dike is of compact basalt, not many feet in thickness, but may be traced a considerable way without much variation of its character. In dip and direction it nearly corresponds with the regular strata amongst which it is placed, being inclined towards the N. E. at an angle of about 60° , and running from 33° E. of S. to 33° W. of N. Its structure is columnar at right angles to the position of the bed. The mica-slate as it approaches the dike assumes a flinty hardness and compactness of structure, and when within some inches of it acquires a dark hue of iron-gray, derived probably from the colouring matter of the trap. Close to the trap the fusion of the mica slate seems to have been completely effected. In some places the contact was so perfect, that these heterogeneous materials have been cemented together, and may be broken as one in a hand specimen; in others, which more resemble the vitrified fort, the mica-slate in a slaggy condition, having come in contact with the trap at some points, has taken an impression from it, and contracted into cavities in the intervals, which appear to contain the matter of the slate nearly in the condition of a crystallizable compound. The characteristic difference of this vitrification from that observed in the neighbouring fort may be contained in the evidence of being the result of *heat under compression*. Instead of the spongy porous mass which the micaceous layers of the rock present in the artificial example,

we find a heavy and compact clay, which seems to proceed from the intermixture of all the component parts of the rock united into a solid mass, completely destroying the original structure, which is not so in the other case. In the centre of the dike occurs an insulated stratum of mica-slate, which is exceedingly altered by the intense heat to which it has been exposed.

I believe that there is no class of phenomena more calculated to throw light upon geological dynamics than that of the trap rocks; nor has the evidence which they are calculated to reveal perhaps been studied in that detail which its importance, and the apparent remoteness of the truths on which it bears, demand. It would be difficult to find laboratory experiments which could more pointedly illustrate the inquiry than the materials of vitrified forts, which, with the exception of modern volcanic eruptions, are perhaps the best evidence we possess belonging to the historic era.

ART. XVII.—*Observations on the Decline of Chemical Science.*

By CHARLES DAUBENY, M. D. F. R. S. Professor of Chemistry, Oxford. (Extracted from his work on the *Atomic Theory*, with Preliminary Remarks.)

WE congratulate the true friends of science and literature on the rapid and favourable progress which has been made in public opinion respecting the decline of science in England,—the degraded state of our literary and scientific talent,—and the imperfect and almost fruitless organization of her intellectual institutions. The eyes of disinterested and patriotic men have been opened to the soundness of the views of Sir John Herschel, Sir Nicholas Harris Nicolas, and Mr Babbage, and the triumph of their exertions is not far from its consummation. Even in the midst of its unparalleled difficulties, the government of the country has directed its sympathies to the science of England, and has begun that great work of regeneration which no previous ministry had either the inclination to attempt, or the talent to execute. The distinctions which have lately been conferred upon Mr Herschel, Mr Nicholas Harris Nicolas, Mr Charles Bell, Mr Ivory, and Professor

Leslie, and the liberality which has been shown to one of the most eminent of these individuals, have met with universal approbation, and have been reckoned a most favourable earnest of the future plans of the Cabinet, and of the future munificence of the Sovereign.

The transcendent gifts, indeed, by which Providence has distinguished one individual in the Cabinet, and the literary acquirements of many of his colleagues, were viewed as a pledge that the paramount interests of knowledge would not be overlooked; and we know that even the philosophers of other countries look forward with delight to the anticipated renovation of English science.

From these causes the illiberal opposition which was made to the warnings and suggestions of some of our most eminent philosophers has begun to die away; and the cries which rose from the dormitories where science slumbered, have been performing their last echoes within the infant walls of King's College.

The really wise and good men who adorn our present universities have not been parties to these discreditable proceedings. They knew the real state of English science: They felt and acknowledged the imperfections of institutions, which it was out of their power to remedy, and which it was not their duty to expose; and they will be among the first to adopt any salutary changes which originate in sound and cautious views. The leading scientific men in the University of Oxford are pre-eminently entitled to this praise, and of this we have a striking proof in Dr Daubeny's newly published and valuable work on the *Atomic Theory*. This able and active chemist, who so well fills the chemical chair in that university, and whose ardour for science is unextinguishable, might have been expected to have been one of those who felt themselves aggrieved by the picture which Sir John Herschel had drawn of the low condition of English chemistry; but with him the love of truth, and the desire to revive the science which he cultivates, were paramount to all personal and national feelings; and he has not only quoted in his new volume the picture drawn by Sir John Herschel, but has candidly admitted its truth without any abatement or exception.

The following is the admirable Note in which he discusses this subject, and in which he also treats of the general question of the decline of science with much candour and justice.

“ ‘ In England, whole branches of continental discovery are unstudied, and indeed almost unknown even by name. It is in vain to conceal the melancholy truth. We are fast dropping behind. In mathematics we have long since drawn the rein, and given over a hopeless race. In chemistry the case is not much better. Who can tell us anything of the sulpho-salts? Who will explain to us the laws of isomorphism?’ See Mr Herschel’s *Treatise on Sound*, printed in the *Encyclop. Metropol.*

“ From the verdict of one so eminently qualified to pass judgment on the comparative merits of British and Continental philosophers as the writer here alluded to, there can scarcely be any appeal; neither will it be denied, that the inferiority complained of by him and others, is in part attributable to the culpable apathy, which the government of our own country has been wont to exhibit with reference to abstract science in general.

“ It may indeed be true, that in the less abstruse and more popular departments of modern inquiry, such as Zoology, Geology, and the like, extrinsic aid from such a quarter might be dispensed with, the zeal of individuals supplying the place of public patronage; but the same does not apply to the mathematical sciences, which can scarcely ever be duly relished, or successfully pursued, without a devotion of time incompatible with the occupations of those who resort to a profession as a means of subsistence, and a concentration of mind on one branch of study, not often found among those who are placed above this necessity.

“ I fully coincide, therefore, with the writers who have followed Mr Herschel in his estimate of the state of science in this country, so far as to regret, as a circumstance which has operated unfavourably, not only upon the advancement of knowledge, but even upon the character of the people in general, the total want of encouragement on the part of government to any researches, save those practical ones, towards which the genius of the British nation is already too exclusively di-

rected. I must, however, be permitted to add, notwithstanding the respectable source from whence the assertion proceeds, that the writer in the 86th number of the *Quarterly Review*, who has taken this view of the subject, appears to have weakened his own case by overstating it; for when he asserts, in corroboration of his opinion with respect to the decline of science, 'that within the last fifteen years not a single discovery or invention, of prominent interest, has been made in our colleges, and that there is not one man in all the eight universities of Great Britain who is at present known to be engaged in any train of original research;' he must surely have forgotten that Mr Herschel, whom he so justly commends, was but lately a fellow at one of the colleges at Cambridge, that Mr Babbage and Mr Airy hold at present the two mathematical chairs in the same university; and that in Dublin, the professorship, which Mr Lloyd at present occupies, was within fifteen years filled by Brinkley.

"Indeed the strongest argument, I conceive, in favour of the writer's position, viz. that the abstract sciences would be promoted by obtaining the fostering aid of government, might be derived from the degree in which they appear to be advanced by such endowments as at present exist; the fact being, that of the individuals who have obtained any considerable reputation for science in Great Britain, there are but few that have not during some part of their lives derived pecuniary assistance and support from their connection with one or other of our public establishments; and with reference to the present argument, we may with perfect propriety put together the emoluments of our universities, and those of the scientific institutions that have more recently started up. The names of Mr Davies Gilbert, Dr Brewster, Mr Dalton, Dr Prout, and Dr Henry, are all that occur to me as exceptions to this remark; of whom the two former are intimately connected with their respective universities, though they do not partake of their emoluments; and amongst the latter there is at least one individual, in whose case a more liberal public patronage would have secured to science the undivided energies of a mind, at present partially withdrawn from it by other indispensable occupations. On the other hand, the names of Herschel, Airy, Whewell, Faraday,

Leslie, Thomson, and Ivory among the living, and those of Davy, Wollaston, and Young, amongst those recently deceased, proudly attest the usefulness of endowments, which 'though,' as Mr Babbage observes, 'seldom sufficient for the sole support of an individual, and very rarely enough for that of a family,' yet, by enabling a few persons to prosecute objects of public utility, without an entire sacrifice of their private interests, serve to prevent the feeble torch of science from being completely quenched by the all-absorbing pursuits of personal aggrandizement.

"These latter remarks may not be altogether misplaced, at a time, when the legislature of this great empire is said to have seriously debated the expediency of discontinuing the *only* parliamentary grant made to either of the two English universities—a sum of about nine hundred pounds, voted for the increase of the nine poorest professorships dedicated to modern science in these national establishments, and which may possibly amount to about $\frac{1}{3}$ th of the sum, which the same legislature annually exacts from the same bodies, in the shape of taxes, on students admitted, or on degrees conferred !"

The candid reader who peruses this note with attention will perceive how completely Dr Daubeny agrees with Mr Babbage and others in his opinions on the general question of the decline of science,—on the culpable neglect of it by preceding governments, and on the absolute necessity of its being directly encouraged by positive endowments.

Dr Daubeny even agrees with the author of the article on the Decline of Science in the 86th Number of the *Quarterly Review*, though he considers that the reviewer has injured his case by *overstating* it. That Dr Daubeny has in this, and in some other minor points, been led into a mistake, and that he and the reviewer are actually of the same opinion, though they express that opinion in different ways, will appear from the following observations :—

When the reviewer averred that within the last fifteen years no *inventions or discoveries of prominent interest* have been made in our universities ; and that at the time when he wrote, not one man *was known to be engaged in any train of original research*, he made a statement capable of being distinctly ex-

amined. Those who deny the accuracy of this statement, are bound to name the *inventions and discoveries of prominent interest* made in our universities, and the *trains of original research* in which any professor had been engaged. Dr Daubeny, who thinks that the reviewer has overstated the case, does not meet his opponent by a distinct counter-statement. He mentions *four* names of eminent individuals as connected with our universities ;—but names, however great, are not *discoveries* ; and it still remains for him to enumerate the prominent inventions and discoveries which these four individuals made when filling the offices which he states them to have held. The reviewer, as Dr Daubeny will admit, could not have forgotten the four eminent individuals whom he enumerates, viz. Herschel, Babbage, Airy, and Brinkley. He, on the contrary, knew their exalted talents, and the prominent services which most of them had at that time rendered to science ; but he never dreamt of considering, nor can he now consider, his statement as at all applicable to their position.—Mr Herschel's discoveries were made when he had no connection whatever with Cambridge, and Mr Babbage's before he was elected Lucasian Professor ; and from these facts, the reviewer had in the same paragraph, enumerated Mr Herschel and Mr Babbage as persons whose discoveries were not made in universities. The discoveries made by Mr Airy of any prominent interest have been made known *since* the article in the *Quarterly Review* was written ; and we are not acquainted with any prominent discoveries made by Dr Brinkley while he filled the chair which Dr Daubeny says is now occupied by Dr Lloyd. The reviewer had reason to know well the fine observations on parallax and refraction, &c. which Dr Brinkley made as Astronomer Royal of Ireland ; but astronomers have not yet agreed to characterize these observations as prominent discoveries, even if they had been made, as they were not, in his capacity of a professor.

The reviewer's position remains, therefore, untouched by any thing that Dr Daubeny has said ; and in the name of the reviewer we challenge any individual to disprove his statement, by distinctly naming the *inventions and discoveries of prominent interest* which have been made, and the *trains of origi-*

nal research which are *known* to have been carried on in our universities within the period above-mentioned. If such a distinct reply is made, we pledge ourselves to answer it. The motive of the reviewer could not be mistaken. His object was not to vilify our universities. It was to make out a strong case to rouse an ignorant and unfeeling government to a due sense of the importance of science to the interests of the country,—it was to improve the condition of professors themselves, by removing the necessity of their pursuing the commerce of science; and to elevate scientific men in general, and advance the interests of knowledge, by obtaining for them honours and emoluments which were freely extended to them in every other country. Had the object of the reviewer been otherwise: had he intended to injure our literary institutions, he would have used a very different language, and touched upon very different topics; and he hopes, that, in the defence of a position, yet unassailed, and, as he believes, unassailable, he may not be under the necessity of raising new outworks, which may prove still more impregnable.

But, admitting the correctness of Dr Daubeny's mode of viewing the subject,—where is the real difference between him and the reviewer? The only exceptions to the reviewer's statement Dr Daubeny finds in the discoveries of four philosophers. The reviewer, too, admits the value of their labours, so that the only difference between them is, that Dr D. conceives himself entitled to view these discoveries as the products of their university connection, while the reviewer conceives them to be totally independent of all such connection. Upon the principle adopted by Dr Daubeny, Scotland may claim the merits of Sir John Herschel's discoveries, because his mathematical talents were first fostered by a Scottish mathematician. Cambridge might also claim the labours of Dr Brinkley; and our unpretending University of St Andrews might rear its head above them all, and claim the talents and genius of Playfair, Leslie, and Ivory, who were all educated within her ancient walls.

In point of fact, Dr Daubeny does entertain some peculiar notions on this subject which are to us utterly unintelligible, as he goes on to state, that Mr Davies Gilbert and Dr Brewster

“are *intimately connected with their respective universities*, though they do not partake in their emoluments.” We do not know the nature of the connection which subsists between Mr Davies Gilbert and the university to which he belonged ; but we can assure Dr Daubeny that there is no connection at all, and still less an intimate one, between Dr Brewster and the university of Edinburgh, except that he was educated within her walls, and paid the fees for his degree of Master of Arts. Dr Prout and Dr Henry were, we believe, similarly connected with the university of Edinburgh.

We heartily agree with Dr Daubeny in reprobating the idea of government withdrawing, without sufficient reason, the nine pensions of L. 100 each, which are allowed to the nine poorest professors of the English universities ; and we earnestly hope that no such scheme has been proposed ; but we must at the same time express the hope that none of these nine professors are among the number of those who, in the controversy about the decline of science and the means of its revival, have privately and publicly maintained that science should not be directly encouraged by the nation,—that scientific men should not receive pensions from their sovereign ; and that the dignity of science is compromised by any commercial connection with the state.—If, on the other hand, some or all of those nine professors have been *actively* opposed to the promotion of the interests of other men of science and literature, who are not professors ; while they are themselves *inactive* in the promotion of their own branches of literature and science, or of any other branches equally useful to the nation ; it might then be a fair subject for the consideration of a wise and just government, whether the annual sum of L. 900, which has been generously advanced to promote the interests of learning, may not be applied with much better effect in encouraging active research, and in fostering unprotected talent. We trust, however, that no such step will be taken, and if we had any influence, we should recommend that the grant be *doubled* rather than diminished.

ART. XVIII.—*The Volcanic Basin of Rieden, in the Lower Rheinland.* By S. HIBBERT, M. D. F. R. S. Ed. &c. &c. Communicated by the Author from a volume unpublished, "*On the Ancient Volcanoes of the Basin of Neuwied, &c.*"*

THE geographical situation of the Basin of Rieden will be immediately recognized by the geologist when I state, that it is to be found in the ancient volcanic tract of country which bounds the left bank of the Rhine, between Coblenz and Andernach, and that it is farther situated about two or three miles to the west of the Lake of Laach.

The limits of the valley of Rieden, which are so irregular as nearly to baffle description, exhibit on all sides bold declivities, or in the place of them, abrupt and insurmountable precipices. Nor are the inequalities of surface which characterize its recesses less striking. These, when contemplated from an eminence, such as that of the commanding point named the Ganschalls, exhibit such a multitude of insulated cones, of towering peaks, or of deep and narrow ravines, the whole clothed with almost impassable thickets, that the mind appears perfectly lost, and it seems at first view the most hopeless of tasks to thread with any degree of geological accuracy this perplexing and almost inextricable maze.

This is the sole reason that the district which I would now describe appears nearly a blank in all the dissertations which have yet been published of the volcanoes of the Rhine. "In no other part of these confines," says one of the latest geological visitors of the Rheinland, "have the deluging effects of waters and currents in connection with volcanic eruptions, with subsidences and trachytic elevations, occasioned a greater confusion than in the neighbourhood of Rieden, Weibern, and Kempenich. We can observe only the trachytic Burgberg, the half defaced crater-shaped cavity, the destroyed and displaced basaltic lavas, the great fragments of trachytic leucite which lie scattered upon an ovenstone, (indurated tufa) disposed in strata, or which are inclosed in it,—and the deposit of

* This communication was read at a meeting of the Royal Society of Edinburgh.

the most recent sandstone formations which alternate with volcanic ones. Every thing here appears chaotic and intermingled. Without new topographical observations, connected with elevations, it is not possible to give a distinct representation of this district." (*Uebersicht, &c. &c. von H. J. Freiberrn vander Wyck.*)

In despairing sentiments like these I was myself at first disposed to indulge. But after some little observation, the great difficulties to be encountered reduced themselves to the following: The first, which was by no means a small one, arose from the dense state of the forests and thickets. This I readily obviated by returning to the site early in the spring before the foliage had appeared; while the second, which was the complex geography of the place, I subdued by a regular survey of the ground. *

After these observations, I shall now attempt the very difficult geology of the basin of Rieden.

The history of the basin of Rieden may be summed up in a few words.—It has been originally a volcanic crater, probably induced during the incipient liberation of elastic gases:—Secondly, it has been the seat of trachytic eruptions:—Thirdly, it has been filled with a tufaceous mud:—Fourthly, this tufaceous mud has overflowed into adjacent vallies or lakes:—And, fifthly, it has been the seat of later basaltic eruptions.

These circumstances being premised, we shall consider each volcanic incident in the order of time which it appears to have observed.

SECTION I.—THE ORIGIN OF THE BASIN, OR CRATER, OF RIEDEN.

There is every reason for supposing that the basin of Rieden had its origin during some elevation, perhaps a renewed one, to which, about the commencement of the tertiary epoch, the westerly bounding hills of the valley of Neuwied became subject. It probably formed one of the many circular fissures occurring in this volcanic district, which was induced by gaseous fluids given out during the secular consolidation which

* A map of the Basin of Rieden appears in the unpublished volume.

the interior fluid mass of the globe has undergone ; and which, in exerting a powerful uplifting force for their extrication, has formed in many places deep and yawning rents. In consequence, however, of a long series of volcanic eruptions, the original shape of the cavity has been much distorted. It is notwithstanding capable of being traced, and appears to have been nearly of a circular form, having a diameter of about a mile and three quarters to two miles.

SECTION II.—THE TRACHYTIC ROCKS OF THE BASIN OF RIEDEN.

The numerous rents which the mountainous plateau where this basin had occurred must have sustained from the uplifting force of gaseous fluids, was followed by the escape through them of felspar in a liquid state.

The various products which appear to have risen from the deep aperture of this volcanic basin form the rocks which geologists name *trachytic*,—that is, they have a base of felspar, and include in them in various proportions crystals of glassy felspar ; they are also in different places more or less modified by the presence of hornblende, augite, or leucite. The outward limits, however, of this great fissure, are distinguished by eruptions of basalt, but as these are of a later date, they demand a subsequent and distinct consideration.

The varieties of the trachytic rocks to be met with in the valley of Rieden, particularly on the south or south-easterly side of it, are numerous. They are as follows :

1. A volcanic rock, having a base of felspar of a greenish grey colour, and of a compact texture, which has evidently suffered more by the action of heat than most of the other varieties to be found in this locality. For, among the numerous small crystals of felspar which the base contains, some are in a vitreous state ; others are either reduced to the state of a kaolin, or to that of a whitish powder, while the rest have been so affected by the intensity of volcanic fires that the substance of them has nearly disappeared ; small corresponding pores being left as memorials of their prior existence. This rock likewise contains many small fragments of clay-slate entangled by it during its propulsion.

2. A second rock which is to be found on the south-east of the valley has a base of felspar varying from a fawn colour, which is the most prevalent, to one of a yellowish and hair brown of a darker shade. The texture is compact. Minute crystals of glassy felspar and hornblende are sparingly disseminated through it.

3. The base of another rock is of nearly a similar colour to the last, the shade being perhaps darker. But its texture is granular rather than compact; or, to explain this character, when viewed through a microscope, it exhibits countless granular particles of a darker coloured felspar disseminated in a base of a lighter colour, and intimately blended with it. The structure of this rock is also inclined to the schistose. Crystals of glassy felspar, some of which pass into the state of a dull white kaolin, and those of hornblende are even fewer than in the last variety.

4. The feldspathose base of a fourth variety is of a darkish hair brown colour, with a compact texture and a schistose structure. It contains a few crystals of glassy felspar, but chiefly differs from the preceding varieties in the greater diffusion through it of crystalline particles of hornblende.

5. A fifth variety has its texture characterized by a commixture of minutely granular particles of an olive green colour, but differing in their degrees of shade. Crystals of hornblende and glassy felspar are sparingly observable in it.

6. A sixth variety, which is a very abundant rock in this valley, has a bright greenish grey base of felspar with a compact texture and a very uneven fracture. I have been able to detect in it but few crystals of glassy felspar, and no augite or hornblende. It is chiefly remarkable for the innumerable particles of leucite, about the size of millet-seed, which are imbedded in it, but which are so intermingled and confounded with the feldspathose substance of the base, that they have scarcely preserved more of their forms than pale-coloured outlines, most of these even being very imperfect. It is, however, possible to detect a few perfect crystals of leucite, amounting even in size to that of peas. This rock is extremely decomposable.

7. A seventh variety has a base of felspar of a dark yellow-

ish grey colour, with a very compact texture, and a structure slightly conchoidal. Like the last described rock, it contains numerous small indistinct particles of leucite, but along with them minute yet perfect crystals of glassy felspar, and of augite or hornblende,—it is difficult to say which. This rock, from the effects of heat and sudden cooling, may be observed in a few places to pass into a dark-coloured pitchstone, varied by minute white spots, the indications of included particles of leucite.

8. An eighth variety has a base of felspar of various colours and shades. The colour of one is clove brown mixed with ash grey and a small tinge of red, forming altogether what Syme in his Wernerian nomenclature names a broccoli brown. In other instances the felspar is of a bluish or blackish grey rather than of a broccoli brown tint. The texture of the rock is minutely granular. It is also densely studded with crystals of leucite, varying from the size of millet seed to that of small peas, and these exhibit various tints from a wine yellow to that of a hair brown. Small crystals having rather the character of augite than of hornblende are almost as abundant as the leucite, and along with them are crystals of glassy felspar, some few of which are of the dimensions of half an inch or more. In the bluish or blackish grey specimens I found these last mentioned ingredients more proportionally interspersed. Lastly, this volcanic product contains in it fragments of the primary rocks through which it has been protruded, as, for instance, granite very materially altered by heat, large scales of black mica, &c.

9. A ninth variety has a felspathose base of a bluish grey colour, chiefly remarkable for its crystals of white leucite, which occur in an abundance that is perfectly remarkable, forming in some parts of the rock a great portion of its substance. Next in quantity to the leucite is the augite, which enters intimately into the substance of the base. I could not detect in this variety any glassy felspar.

10. A tenth variety, which is not an abundant rock, has a lavender purple base of felspar. In its texture it is granular, and also contains numerous small pores. Crystals both of hornblende and augite, chiefly the former, enter into it, which

are much larger than in any other rock of the valley, some of them being of the dimensions of the fourth of an inch. It also contains, though very scantily, crystals of leucite.

11. An eleventh variety has a blackish green base of felspar, in which numerous crystals of hornblende are disseminated, to which it probably owes its particular colour. In its texture it is minutely granular. Glassy felspar is with difficulty to be detected in it. Crystals of leucite varying from the size of a common millet-seed to two or three times that magnitude are interspersed through it, though very scantily; these being far less decomposable than the base in which they are contained, may be observed in weathered specimens to fall from it in distinct crystals.

12. A twelfth variety has a felspathose base of a greenish black tint, which it doubtless owes to the colouring matter of hornblende, but it differs from every other rock of this valley yet mentioned in its compactness and hardness, which is equal to that of a basalt or greenstone. When examined through the microscope many very minute spots of a whitish substance may be found interspersed through it, which I have been in doubt whether to consider as molecules of leucite, felspar, or even quartz;—perhaps they are of the first named ingredient.

13. The remaining variety to be described is one that resembles the last in the numberless minute spots which are to be detected in it by the microscope, and which, on decomposition, certainly appear to most resemble leucite. But it has a different colour, namely, that of a reddish or liver-brown, and it is of an inferior compactness and hardness. It also contains small crystalline particles of hornblende or of augite, and minute scales of mica.

From the list now given of the more ancient volcanic rocks of the valley of Rieden it will be seen, that they have all a base of felspar more or less diversified by the presence of glassy felspar, hornblende or augite, and that many varieties have to boast of the diffusion in a remarkable quantity of crystals of leucite. By some writers other minerals have been recorded

as occurring among these rocks ; as, for instance, Nosin and Hauyne.

It may be also added, that many of these varieties pass into each other by almost imperceptible gradations.

The structure of the trachyte is nowhere remarkable. I have not seen it prismatic. It is in general divided by seams into polyedrous or rhomboidal fragments.

M. Steininger has noticed volcanic balls strewed about the vicinity of this basin between the Gansehalls and the Selberg, which he supposes to have been erupted, along with ashes, from the first mentioned place. These balls he describes as having a sort of *wacke* appearance, and as containing glassy felspar, leucite, mica, and hornblende, and even melanite and spinelle.

Fragments also of various kinds of trachytic rocks are found scattered on the ground near the Gansehalls, and even, according to some writers, fragments of opaline felspar.

In this description of the felspathose rocks of Rieden, it was my wish to have appended to them the names which have been recommended by some distinguished geologists. But, whether from my own unskilfulness, or from some imperfection in the classifications themselves, or perhaps from both causes together, I have certainly found the task beyond my ability. I shall therefore content myself with remarking, that most of the varieties detailed may, I conceive, be referable to the trachytes of Brongniart, with the exception perhaps of a few of them, which may have bases approaching to the Diorites, Eurites, Ophites, or Argilolites of systematic writers. At any rate, I have attempted to define each variety of rock with such accuracy, that those who are strenuous for more particular systematic names can scarcely fail to find sufficient data for their purpose.

The nature, likewise, of the primary rocks through which volcanic streams have been protruded, is displayed. Ejected fragments of granite are observable, one of which, collected by me, has a whitish, or yellowish coloured base, containing large crystals of felspar, which have been reduced by heat to a vitreous state, and small specks of hornblende. The mica is in minute quantity.

Such are the varied products of Rieden, which are mostly

found in a lithoid state. The slaggy form is indeed a comparatively rare occurrence.

The conditions under which these rocks have been erupted are well displayed by their present striking appearance and distribution. Incipient rents were soon followed by the protrusion through them of enormous columns of trachytic felspar. These, when they appeared above the deep confined abyss, formed isolated or even connected cones. For, as their ascent took place near the bounding walls of the basin, they would either rise in the form of high and surmounting peaks, or in viscid streams would seek the low and confined depths of the valley, which they would choke with thick and sluggish accumulations of lava. A state of quiescence would be thus induced, which would afford the condition considered by Von Buch as indispensable to the formation of leucitic ingredients.

From these circumstances we are led to infer the general appearance which would be presented upon the first breaking out of these trachytic rocks. If our imagination is capable of transporting us to a period apparently antecedent to the habitation of Europe by the human race, we should find, that, upon attaining the summit of the high plateau of clay-slate, within the bowels of which the present wooded valley of Rieden now appears, we should perceive ourselves upon the brink of a hideous precipice, looking down upon a burning lake 500 or 600 feet below us, contained within a vast irregular fissure, varying in its dimensions from two to four miles, while several conical islands formed of the same incandescent matter, would be elevated above the fiery flood. The appearance would, in fact, find something like a recent analogy in the volcanic phenomena of Owhyee, lately described by Mr Ellis the missionary. This author states, that, upon approaching the volcano of Kiraula, he expected to have seen a mountain with a broad base and rough indented sides, composed of loose slags or hardened streams of lava, and whose summit would have presented a rugged wall of scorix, forming the rim of a mighty caldron. But instead of this, he adds, he found himself on the edge of a steep precipice with a vast plain before him, fifteen or sixteen miles in circumference, and sunk from 200 to 400 feet below its original level. The same traveller then continues to describe the surprise which he felt, while

descending amidst these vast lava-fields, to find in that portion of it where the volcanic energy was still active, an immense gulf, (to use the author's own expression,) where was a vast flood of burning matter in a state of terrific ebullition, with numerous conical islands rising round the edge or from the surface of the burning lake.

But it is now important to add, that, if this description be at all relevant, it can only apply to this ancient volcano of the Rheinland in its incipient stage. The beds of tufa of an immense thickness, which in every part of the cavity are superimposed upon trachytic rocks, present indications of the early site of a volcano of mud, or *moya*, similar in its character to those appalling ones of the western world, the ravages of which have been commemorated by Humboldt.

SECTION III.—THE TUFACEOUS DEPOSIT ACCUMULATED WITHIN THE BASIN OF RIEDEN.

It may be here stated, that in the attempt to investigate, in an order of time or succession, the appearances presented by the valley of Rieden, an early priority of description has been allotted to rocks of trachyte, which ought, however, to be accompanied with this explanation, that we are not warranted in concluding that they were protruded from the volcanic focus exactly at one time; the greater probability being, that the various volcanic cones or congealed flows of lava displayed within the basin were the result of a series of eruptions continued during the formation of the tufa.

Either contemporary with the fiery floods which were thus confined within an abyss of the mountains, or subsequently to the period when they had begun to roll, torrents of water from each commanding eminence would descend into the basin, and would give rise to new and tremendous phenomena.

These it will be our next study to investigate.

It is well known, that, besides the protrusion of lava in a flowing and viscid state, light pumiceous matter forms a considerable portion of volcanic products. In most of the extinct volcanoes of this character which I have studied, the nature of their tufaceous deposits has convinced me, that pulverulent par-

ticles of trachyte, generally of a milk or yellowish white colour, and intermingled with minute portions of pumice, are so frequent an accompaniment of trachytic eruptions, as to almost merit being considered as one of their characteristics. From their very minute state of mechanical division, and extraordinary fineness, they readily, when mingled with water, form a paste.

Under ordinary circumstances, these extremely levigated particles, as they issue from the volcanic focus, would be dispersed by volumes of gas and vapour, and be carried into the more elevated regions of the atmosphere, there to become the sport of winds; a very light current of air being sufficient to transport them to a considerable distance.

These conditions being premised, we may readily conceive of the focus of a volcano, in which such minute and light trachytic particles are elaborated, being called into activity beneath a cavity half filled with incandescent lava, into which would soon flow various mountain streams. In this case it is evident, that the superambient waters of the lake would intercept all the light particles of volcanic matter which would otherwise have been projected high in the air, and widely dispersed over a large surface of territory, causing them to be mingled with water of a temperature highly elevated from its contact with incandescent lava. The mixture would then assume the consistence and character of a boiling mud.

There are again other circumstances, though subordinate ones, to be considered, which, while they have promoted the accumulation of tufa, might have possibly conspired in modifying its mineralogical character.

The first of these is the decomposing effect resulting from the increased temperature of the water.

In examining the character of the tufa of Rieden it is evident, that the strong chemical energy which water raised to a high degree of temperature would exert upon earthy substances, must be the most evident in sites where the boiling mud would come in contact with protruded trachytic rocks while in a state of ignition. Now this is remarkably exhibited in the instances where this proximity can be traced. The lowest strata of the valley of Rieden show, that the mud which was deposited upon the surface of incandescent lava has its ingre-

dients often reduced to an extreme state of fineness, indicative of the complete disintegration, and often decomposition, to which any coarse ingredients must have become subject. That this effect is due to a chemical energy increasing with the temperature of the water, is evident from the reflection, that if volcanic ingredients of different degrees of fineness or coarseness had been diffused through a cold watery medium, they would have subsided according to the laws of gravity; the coarser ingredients occupying the lower situation.

There is likewise some reason for supposing, that the same agency of heat has had a general influence in adding to the muddy contents of the basin. Thus, in the instance of a hard leucitic greenstone, which, under ordinary circumstances, was little decomposable, the surface of the rock appears to have passed by almost insensible gradations into the substance of its tufaceous coating.

A second circumstance to be taken into consideration, is the decomposing effect which would arise from the water absorbing many substances, familiar to the naturalist, which issue from the volcanic focus. Chlorine, for instance, hydrogen gas, carbonic acid gas, along with sulphureous vapours in different forms, are well known products of many volcanoes, which, either singly or in combination with alkaline or metallic bases likewise generated, would no doubt be in part absorbed by the lacustrine waters through which they were propelled. Thus, the waters which issued from the cavernous recesses of Etna during the convulsion of the year 1751, were not only in a state of ebullition, but had even acquired saline qualities. Such chemical properties, in fact, does the lake of Laach retain, though very slightly, even at the present day; the labours of the chemist having detected in its waters the presence of the carbonate of soda. Let us then suppose, (what is even more than probable,) that the lacustrine waters of Rieden, besides being intermingled with earthy particles in a state of suspension, were freely impregnated with mineral or saline compounds elaborated during volcanic operations, and there would be doubtless imparted to them a considerable decomposing power, which, in acting upon solid rocks of trachyte, or even upon the larger cinereous or pumice-

ous fragments which were ejected, would not only promote the accumulation of the tufaceous mud, but likewise endow it with new mineralogical properties.

This mud or *moya* (if I may be allowed to use the Transatlantic synonym,) during the progress of its cooling appears to have subsided in the form of a stratified deposit. Sufficient evidence is afforded of this result in the tufaceous coating which still in part conceals most of the trachytic eminences, and which lines the very brim of the containing basin.

To illustrate this fact more distinctly, I have endeavoured in the annexed slight section, (from N. W. to S. E.,) to convey a notion of the basin of Rieden, as it must have subsisted in a state of integrity subsequent to the cooling of the contained *moya*.



But it is necessary to repeat, that the walls of the basin, where the filling of it with *moya* first took place, had a height rather lower than that which they afterwards assumed, particularly on the south, where a drainage of it had commenced. For this reason, the tufaceous strata which are entitled to a priority of description must be sought for amidst such as were deposited antecedent to eruptions of basalt, to which in many places the augmented elevation of the walls is due.

These are the very general observations with which I shall content myself on the origin of the tufaceous strata of Rieden, preparatory to a description of them in their present indurated state.

They are usually of a yellowish or light-brown colour, and are finer or coarser in proportion to the degree with which their ingredients have been enabled to resist decomposition. Thus, we find enveloped in a tufaceous paste composed of minute particles, small portions of trachytic felspar, or of pumice, as well as crystals of glassy felspar, of leucite, or of hornblende, and, in some places, heterogeneous fragments, generally of diminutive magnitude, of the rocks, chiefly clay-slate, through which lava

had been protruded. In other instances, however, the various ingredients of the tufa seem to have undergone a perfect decomposition, and an apparently homogeneous mass of a slaty structure is the result. It may be also observed that these strata exhibit very different degrees of hardness, some being soft, while others are of nearly the consistence of freestone, for the purposes of which they are in daily use.

Some few peculiarities of the tufa may now be noticed.

About half a league south by west of the village of Rieden, on the right of the stream as we trace it towards the present gorge of the valley, a deposit of very fine tufa may be observed resting upon clay-slate;—this being in fact the only site where the common primary rock of the district is found within the basin. The tufa is of a yellowish-grey colour, consisting of the finest possible particles, which are pulverulent to the feel, which, when rubbed on woollen, leave a mark upon the cloth, and which are easily scraped by a knife. In other respects, however, the tufa is cohering and solid. When broken, it is remarkable for showing in various places systems of concentric rings of a chestnut-brown colour, like those of some arborescent stem, which differ in their diameter, the smallest being that of a pea, while the largest is two inches in extent. The rings of each system vary from two to five or perhaps more in number, while, in some of them, there is a sort of nucleus in the centre about the magnitude of a common pea. Whether these appearances can be rationally referred to a vegetable origin, I am yet unable to say.

Contained in this tufa are masses, some feet in extent, of a substance rather different in its external appearance. It is of a pale wine-yellow colour, variegated with almost equally pale yellowish-grey shades. It is, like the tufa which encloses it, very light; its specific gravity being about 1.79. Its great difference, however, consists in its firm and compact texture, and in its clear conchoidal fracture, owing to which peculiarity of structure, as well as its containing systems of concentric rings like those of an arborescent stem, it acquires a faint resemblance to ligneous matter. At the same time, this suspicion is little strengthened by the nature of its chemical ingredients, which consist of silica, alumina, magnesia, and lime. The last sub-

stance might have possibly been derived from calcareous springs issuing through tufaceous matter.

Associated with these substances is a tufa, in which may be traced minute particles of white decomposed felspar, along with others of a deeper shade, of a wax-yellow tint.

In other parts of the valley of Rieden we find a very compact and fine tufa of a bright yellowish colour, which is very fissile, and breaks into laminae of about an inch to an inch and a-half in thickness. It is in some places diversified by layers of a far coarser texture, which contain small fragments of rocks, wherein the substance of clay-slate is perfectly distinguishable.

Elsewhere the tufa is of a coarser texture. It is characterized by particles of a yellowish and light brown colour mixed with minute scales of mica, comminuted particles of hornblende, &c. Upon exposure to a dry atmosphere, it acquires considerable hardness and consistence. It is also disposed into thick tabular masses, often regularly stratified, into which it splits.

A tufa of this character readily recommends itself for economical purposes, and is, therefore, in the valley of Rieden extensively quarried, being named by the natives, from its valuable quality of resisting the effects of heat, *Ovenstone*. When raised from the quarry it is in comparatively a soft state, which is availed of by the workmen for reducing it on the spot to the shape required.

There is again a still coarser tufa which has resulted in part from decomposed trachytic rocks. It is often of a yellowish brown colour, containing in it abundant crystals of leucite, of a very perfect form. This circumstance may arise from the superior power of leucite to resist decomposition, whence its superiority of abundance over crystals of glassy felspar.

These are the principal tufas indicative of the boiling tufaceous mud, or *moya*, which once filled, even to an overflow, the valley of Rieden. Some other varieties might be described, which generally form the higher strata; but as the latter were apparently produced during a later period, they will be described in an ensuing section.

SECTION IV.—THE OVERFLOWS OF TUFACEOUS MUD, OR MOYA, FROM THE VOLCANIC CAULDRON OF RIEDEN.

In the last section the circumstances were investigated under which an accumulation of tufaceous mud, or moya, has been formed. This substance, while in a state of ebullition, must have imparted to its containing cavity the character of a cauldron rather than of a common basin.

After this inquiry we are prepared for the consideration of various overflows of boiling mud which were discharged from the interior of this volcanic cauldron.

But, as a previous and indispensable measure necessary for our understanding the various circumstances under which these different overflows have taken place, it will be important to obtain some general notion of the situation of the basin of Rieden, in reference, first, to an ancient tertiary lake which I have named from the town now existing in the centre of the drained site, The Basin of Neuwied; and, secondly, to certain minor vallies or contemporaneous craters.

To impart to this object sufficient intelligibility, an accompanying plan is attempted of the state of this district at the commencement of the tertiary epoch. It must not, however, be supposed that such a sketch is an hypothetical one. I would rather have it considered, even in the simple form which it now assumes, as one of the ultimate deductions of a very laborious and tedious investigation.

SKETCH ILLUSTRATIVE OF THE TERTIARY GEOGRAPHY OF THE BASIN OF RIEDEN, &c.



Basin of Neuwied.

- Eng. Miles.*
- a* Basin of Rieden.
 - b* Basin of Fusel.
 - c* Basin of Wehr.
 - d* The ancient water-course by which an overflow from the basin of Rieden was conveyed into the lacustrine expanse of Neuwied.
 - e* The ancient lake of Gleiss.
 - ff* A branch of the river Nette from Kempenich.

In allusion to this sketch it will be observable, that the basin of Rieden (*a*) was situated on the margin of the tertiary lake of Neuwied. The bed of this large lake occupied all the low land which we trace along the course of the Rhine from Coblenz to Andernach. Its extent was 16 to 18 miles from east to west, and 4 to 7 from north to south.*

On the north a narrow clay-slate ridge divided the basin of Rieden from that of Vehr, (*b*) also of volcanic origin; and west of the latter, the same clay-slate ridge, subsequently widened by the interposition of an invading volcanic rock named the Lierenkopf, separated it from the Valley of Fusel.

On the east a clay-slate ridge divided the valley of Rieden from the crater lake of the Laacher-see and the basin of Neuwied, the waters of which were then intermingled; and, by a similar interposition on the west, the valley of the Nette (*fff*) was separated.

But the southerly boundary acquires the most interest. It must be carefully kept in view that the height of the cliffs which formed these limits was originally lower than is exhibited at the present day; their elevation having been subsequently increased by overtopping eruptions of basalt. Accordingly, when the walls of the basin of Rieden were comparatively inconsiderable, an eruption of its waters was carried off by a ravine, (*dd*) which conducted them in a south-easterly direction to the basin of Neuwied.—(*This is represented in the wood-cut.*)

After this elucidation, we are prepared for inquiring into the co-operating causes which might have induced the overflows of tufaceous mud which have their origin in the basin of Rieden.

One of the causes giving rise to the overflow of tufaceous mud may be referred to periodical or occasional floods of water swelling the volume of the muddy contents of the basin.

That the basin of Rieden, whether in the state of a common crater lake, or of a volcanic cauldron filled with moya, was

* The geology of the basin of Neuwied was described in a former treatise of mine which appeared in this *Journal*, "*On the Brown Coal Formation of the Lower Rheinland.*" In this memoir, however, I named it the basin of Andernach, from the site of its gorge.

liable to overflows from periodical or occasional floods of waters swelling the volume of its muddy contents, is to be inferred from the deep water-course which may be traced on the south-east of the basin. In examining a portion of this ravine, which near Bell is excavated through rocks of clay-slate, we find at the bottom of it two deposits, the lower of clay, and the higher one of loam, which had apparently their origin before any eruption of volcanic mud had commenced ; while above them is a thick accumulation of volcanic tufa (yet to be described) with which the water-course was subsequently choked up.

But this original drain would only be efficient when the walls of the basin from which the outlet was conducted were comparatively lower than at present. For, in a later period, a change occurred. The source of this drain was obliterated by the intervention of protruding volcanic rocks, which in breaking through the very brim of the crater appears to have added considerably to its height ; compelling at the same time the waters of an overflow to excavate for themselves some new channel of escape.

To render this description of the original or prior state of the basin more intelligible, the sketch of it may be again consulted, where the drain in question will be found indicated by the letters *d, d*.

Having described the appearance indicative of one of the causes to which many overflows of moya into continuous vallies may be referred, I shall now advert to another co-operating agent.

Although a considerable share of the tufaceous mud thus deposited may be supposed to have owed its origin to accessions of water swelling the volume of the muddy contents of the basin, and thus causing an overflow, yet it is certain that another cause must have still more conspired in producing the same effect, namely, renewed eruptions of lava within the actual confines of the basin. In this latter case it is evident, that a considerable volume of tufaceous mud would be displaced, and that, in its farther elevation by the expanding force of the elastic gases which would be liberated, it would boil over the sides of the caldron, and be precipitated into contiguous vallies.

To such a cause I would certainly refer some share of the overflow which has been described, consistently with the obser-

vation which I have made, that we are not warranted in concluding that the trachytes of Rieden were protruded from the volcanic focus exactly at one time; the greater probability being, that the various trachytic cones or congealed flows of lava displayed within the basin were the result of a prolonged series of eruptions.

With these few general observations I shall now content myself, and, in proceeding to describe the overflows of moya incidental to the caldron of Rieden, such as have found a lodgment in deep and dry vallies or ravines will be distinguished from others which have been ultimately conducted into contiguous lakes.

1st, The overflow of tufaceous mud, or moya, which was conducted by the ancient water course on the south east of the basin of Rieden into the lacustrine expanse of Neuwied.

There is every probability that, owing to various periodical or occasional accessions of water imparted by mountain torrents to the basin of Rieden, it would be subject to a gradual process of overflow, accelerated only by the sudden discharge of moya which would ensue when protrusions of lava intervened.

During this process, much volcanic mud would flow over the south easterly walls of the basin, which at this point appear to have rapidly declined towards the great lake of Neuwied; and having been transported along the line of the ancient ravine, which I have described, would be extensively diffused among lacustrine waters.

From this intermixture a very modified deposit would be the result. The substance of the tufaceous mud would be so intimately blended with the fine sand and plastic-clay, which are the characteristics of this tertiary lake, as to assume a less firm consistence, which at the present day is indicated by the rapidity with which the mixed deposit is disintegrated, particularly by the action of rains. But although the tufa is thus disguised, its origin, after a little experience, may be easily recognized, owing to the yellow or brownish colour, and pulverulent, yet harsh feel, which it continues to preserve.

There is also abundant evidence to show, that the moya which

was thus washed into the lake became acted upon by currents, particularly from the west and south-west, and that, having been distributed over a surface of several miles in extent, it has served to fill up considerable depressions, where it has been deposited in the form of regular strata. It may likewise be observed, that the lighter particles have undergone the most distant transportation, whence the greater fineness of the product in proportion as we recede from the source of the mud eruption.

The sites which are at present filled with the remains of this mixed deposit, may be found upon the south-west and south of the margin of the Laacher-see, where various small hills or knolls attest its presence. Indeed, there is reason to suppose, that some of it was carried into the lake of Laach itself, (which then mingled its waters with those of the Lake of Neuwied,) though at an advanced period of the plastic-clay deposit, which on the north of this crater appears pure, and unmixed with tufa.

The greatest share of this tufa, however, was not transported easterly towards the Laacher-see, but rather in a south-easterly direction towards the present site of Obermendig and Thur, a distance of three or four English miles, where the deposit lies very deep, particularly in the vicinity of the last named place.

2dly, The overflow of tufaceous mud which was lodged in the ravines of the Ganschalls and of Bell.

During one of the later eruptions of Rieden, which is perhaps indicated by some trachytic eminences, observable on the south-east of the basin, a large overflow of tufaceous mud took place; no doubt in a state of ebullition, the progress of which we shall next endeavour to trace.

In the course of this convulsion, the volume of tufaceous mud, which, in a cavity already filled to the brim, had been displaced by some new protrusion of trachytic lava, would find a ready escape through the breach in its walls, which had been effected by the corrosion of the water-course to which I have before alluded.

But so immense was the volume of the displaced mass, that, after rapidly descending in the course of the furrow which mountain torrents had wrought, it at length became, in proportion as the steepness of the declivity lessened, gradually moulded, as it

were, into the trough-shaped ravine which was its recipient, and before it could reach the waters of the Lake of Neuwied, had its course finally impeded by high projecting slate-rocks. Here it found a quiescent lodgment.

The tufa in hardening now appears as two shapeless unstratified mountain heaps, although there is little doubt but that originally these were continuous; the separation having been gradually effected by corroding streams. The substance of it is named by the Rheinlanders *Ovenstone*, indicative of its power of resisting heat, and of the economical purpose to which it is applied.

The more northerly mass of ovenstone derives its name from its commanding eminence, the foot of which it lines, being usually styled **THE OVENSTONE OF THE GANSEHALLS**.

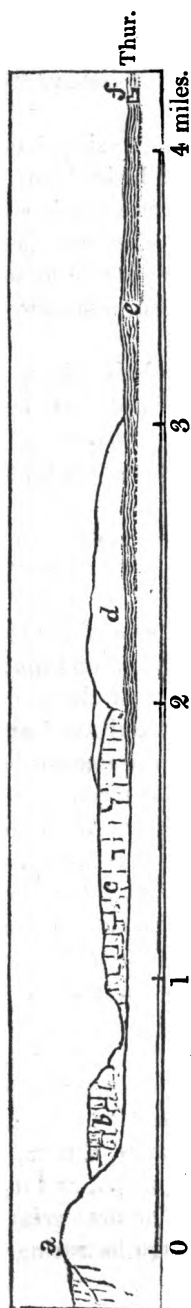
The more southerly mass is distinguished as **THE OVENSTONE OF BELL**, from its vicinity to the village of that name.

The conjoined length, from north-west to south-east, of these two masses of ovenstone may be rated at about a mile and a-half, while their breadth, which is very irregular, does not perhaps exceed a third of this extent. The thickness of the ovenstone varies much owing to the inequality of the ground upon which it has been deposited. It is the greatest at the Gansehalls. Near Bell, where it thins off, it has been estimated at fifty to seventy feet; but I am doubtful if this measurement is not rather confined to the depth of the workable mass.

Whether any portion of this substance at the time of its eruption was washed away into the lake of Neuwied, we have no evidence to show, although the affirmative is rendered highly probable. It is indeed difficult to conceive of any eruption of mud so great as this, without supposing that much of it would ultimately be swept into contiguous waters.

Such is the brief history of the origin of the tufaceous eruptions of Bell and its vicinity, which will perhaps meet with additional elucidation from the following section. It is drawn from the highest point of the crater-walls of Rieden, not far from which the overflow of moya had its origin, and it ends at Thur, a distance of four miles, where there occurs a great deposit of the tufa, which had been diffused through lacustrine waters.

SECTION FROM THE BRIM OF THE BASIN OF RIEDEN TO THE TUFACEOUS DEPOSIT OF THUR.



a Summit of the Gansehalls; being the highest point of the brim of the Basin of Rieden.

b The Ovenstone of the Gansehalls; being the northerly portion of the mass of tufaceous mud which had flowed over the brim of the basin.

c The Ovenstone of Bell, or the southerly portion of the same mass of tufaceous mud.

d The Clay-slate Hill which checked the advance of the great mass of tufaceous mud.

e The tufaceous strata, which, having been diffused through the lacustrine waters of the Basin of Neuwied, were deposited as far as the present site of the village of Thur.

f The village of Thur.

After this very general account of the origin and relative situation of the Ovenstone, the rock may be now considered in detail.

The junction of this tufaceous mass, with the subjacent clay and loam, which had previously subsisted in the ravine, is characterized by an agglutination of various mountain fragments, which, during its course, it had entangled in its substance. To this conglomerate mass, which disappoints the hopes of the quarrymen, the significant name of *Dielstein* is given.

Another appearance presented near Bell, which I did not myself observe, but which I have collected from M. Steininger's rather perplexed description of it, is as follows: The great mass of tufa, in insinuating itself into the ravine, appears by its pressure upon the older beds which it covered to have caused the lower one of clay to start up, which, in its displacement, has insinuated itself on each side of the great volume of the tufa, so as to form a sort of thin layer wedged or jammed in between the overlying mass and the walls of the recipient fissure.

The proof that this overflow of moya was a very sudden one, is shown in the total or nearly total absence of all marks of stratification.

It is probable also that its consolidation was a process proportionally rapid. Thus, for instance, in an eruption of liquid mud, attended even with floods of boiling water, which, so late as the year 1631, had its origin in a subterranean lake of Vesuvius, Scipione Falcone, a writer of that time, has affirmed, that even after a few days had elapsed, the mud had acquired an induration equal to that of a solid rock, and that to break it the assistance of picks was necessary.

The mineralogical character of the tufa is various. The *Dielstein*, which generally occupies a low place, has been already described.

A second variety not so coarse has a base of a greyish, as well as of a hair-brown colour, in which numerous small fragments are interspersed of clay-slate or grauacke slate, of scales

of mica, or of light yellow pumice. Some of the fragments are even in a scorified state.

A third variety has a similar brownish-coloured base, but contains no coarse fragments. Leucite is disseminated through it, though generally in so decomposed a state, that its true character is with difficulty appreciable.

A fourth has a base of a greyish-white milk colour, containing decomposed crystals of felspar, and some of leucite, with small undecomposed fragments of other rocks, generally of clay-slate or of trachyte.

A fifth has a base of a yellowish-grey colour, in which rather fine particles of decomposed felspar, of clay-slate, and even of mica, are discernible; the whole forming a rock which has a structure nearly granular, and not very unlike some of our freestones. It is considered as a valuable diversity of the ovenstone, being also well adapted to various purposes connected with domestic architecture. In some specimens which I collected of this rock, I found a tendency to a laminar arrangement.

All these varieties are soft when first extracted from the quarry, and are therefore wrought upon the spot. They subsequently harden, losing at the same time the deepness of their colour.

This rock, as I have observed, is not stratified. It is traversed by cross seams, which resolve it into large angular divisions, often of a very irregular form.

Much of the general contour of these tufaceous mountain masses has been lost during the process of quarrying. Spacious caves are hollowed out in them, the roofs of which are supported by pillars about six or seven yards high.

3. The lesser overflow of tufaceous mud deposited south of the Ovenstone of Bell.

It was probably during the considerable overflow of moya, which I have last described, that a small stream of it appears to have found a lodgment a little to the south of the ovenstone of Bell in a ravine or water course, which was perhaps a distinct and more southerly one, but which, farther east, joined that which is now gorged with the larger deposit. From such a point of confluence the common channel would lead to Ober-

mendig and Thur, where the deposit of tufa is found, which had been ultimately diffused among the waters of the basin of Neuwied.

This lesser deposit, which lies about a mile west of Obermendig, is so inconsiderable, the volume of it not boasting many yards in extent, that it merits little notice. Its colour is generally yellowish grey, and it has a consistence or hardness less than that of the ovenstone of Bell. It is evidently compounded of minute or pulverulent particles of trachytic felspar, and in its composition I detected very small portions of white pumice.

4. The overflow of tufaceous mud deposited near the Hohenstein.

The Hohenstein is an eminence of clay-slate, the seat of later volcanic explosions, rising to the south-east of the basin of Rieden, between which there is a small irregular valley, where, in following its windings, we detect a trifling overflow of moya. It is chiefly lodged in a depression to the south west of the Hohenstein, between this hill and the lofty crater walls of the Hoch Simmer.

This deposit is in some places stratified; whence the inference that its overflow may be attributable to a sort of boiling over, which might have occurred during the earlier accumulation of the muddy contents of the caldron.

The varieties of tufa here displayed may be briefly noticed. Some of them are remarkable for their similarity to such as are to be found among the lower strata of Rieden.

One variety of tufa is of a yellowish-white colour; of a very fine laminated structure,—brittle also like half-burnt pottery-ware.

A second, a little coarser, and of a yellowish-grey colour, approaches that variety which I have described, (see page 120,) as containing systems of concentric rings of a chesnut-brown colour, like those of some arborescent stem. Here, however, the rings are fewer in number, and they appear more like the sections of seeds of the size of a pea.—But, as I have remarked, a vegetable origin is doubtful.

A third variety has a pulverulent earthy structure, which is indicated by its soiling woollen when rubbed against it. Some

portions of it show an internal prismatic arrangement, like that of dried starch;—the prisms, which are columnar, assuming a similar magnitude and form.

A fourth, and rather coarse variety, approaches to the character of the ovenstone. It is somewhat granular, and contains diffused through it minute portions of trachytic felspar, of clay-slate, or of white pumice.

It is not necessary to dwell longer upon this very insignificant deposit, which is only found in small patches.

5. The overflow of tufaceous mud into an ancient lake extending from the Gansehalls to the Lummerfeld.

We trace a very ancient valley which has a course nearly parallel to the long diameter of the lake of Laach, from the north-east of the high ridge of the Gansehalls to the hill of the Lummerfeld, in the direction of S. W. and N. E.

High ridges of clay-slate bound this valley on the east and west, by which it is separated on the one hand from the Laacher-see, and on the other from the basin of Wehr. Its length is about five and a-half English miles. Its breadth varies much, being near the Gansehalls scarcely more than the third of an English mile; and near the Lummerfeld, where it widens about a mile.

Its depths, particularly about half way between the Gansehalls and the Lummerfeld, was originally considerable; and hence the suspicion which arises, that it was a deep fissure induced during some extraordinary convulsion which has taken place probably at the commencement of the tertiary epoch. It must have existed originally in the state of a mountain lake.

It may, for the sake of distinction, be named THE VALLEY OF GLEISS,—from the village of that name which is situated on its westerly side. (*A portion of it is represented in the wood-cut page 122, where it is marked l.*)

From the basin of Rieden this site of an ancient lake is separated by a very narrow ridge; and hence the bed of it must have early received much tufaceous substance from the frequent boiling over of moya, to which the caldron has been subject. Evidence of a remarkably deep and stratified deposit of this kind may be traced below the westerly flank of the Veitskopf.

This tufaceous substance, from its having been diffused through lacustrine waters, much resembles that which has been deposited under similar circumstances in the basin of Neuwied. It is perhaps in the present instance less mixed with sand, but more with fine clay, and hence its brighter yellow tint, its more pulverulent and earthy aspect, and its still more friable and loose consistence.

But from subsequent drainages of the lake of Gleiss, much of its tufa has disappeared, while, from later basaltic eruptions, the character of it in several localities has been considerably modified.

6. The beds of Tufa which lie to the north and west of the Basin of Rieden.

The earlier eruptions of tufaceous mud to which an evident origin can be assigned have been enumerated. There were no doubt many more which it would be difficult to distinguish from such as might have had a different local origin. It must be kept in view, that during the same epoch two additional and contiguous volcanic craters, namely, of Vehr and of Fusel, gave out mud eruptions, which were probably intermingled with those of Rieden. This is perhaps the origin of some of the beds, resembling the tufa of the interior of the basin, which are deposited thickly on certain contiguous or marginal sites, as, for instance, on those which are to the north-east near Vehr, to the north near the Lierenkopf, or to the west near Wiebern.

SECTION V.—THE ERUPTIONS OF EARLY BASALT WHICH TOOK PLACE AROUND THE BASIN OF RIEDEN.

In a preceding section the circumstances were investigated under which an accumulation of tufaceous mud, or *moya*, in a state of ebullition, has been formed within the cavity of Rieden, sufficient to overtop its highest trachytic cones, and to entitle it to the name of a caldron rather than to that of a common basin.

The causes also, referable in turns to mountain floods and to renewed volcanic eruptions, have been pointed out, which have been followed by various overflows of boiling mud. These I have attempted to trace not only in the lodgement which they

might have formed in ancient ravines, but even in their diffusion through lacustrine waters; and in their subsequent deposition.

The latest changes which the basin of Rieden was doomed to undergo arose from renewed eruptions, which were not of trachytic felspar, but of basalt; while the site of them was not in the interior, but in the outskirts or circumference of the caldron.

It has been supposed that, in general, the earlier products of volcanic convulsions were trachytic, so named from their consisting of felspar of a peculiar roughness or harshness to the touch; and that to these have succeeded eruptions of basalt.

The cause of this very frequent succession, for it is not exactly a constant one, is of most difficult solution. It no doubt bears a reference to the deep-seated and internal constitution of our planet, upon which, in the limited pages of this dissertation, I cannot afford any observations.

These basaltic eruptions do not appear to have taken place in the interior, but around the circumference of the basin of Rieden. This is a circumstance of perhaps no very difficult explanation. It is not easy to conceive how a basin of this kind should have been formed during the operation of elevating volcanic forces, without supposing that many marginal fissures would be a consequence of the strain; and as the trachytic protrusions which had taken place through the rent of the crater must have been hardened or consolidated, they would afford an effectual resistance to the eruption of new columns of lava, which would now find their elevation, and escape to the surface of the earth most favoured through the medium of these marginal rents.

No fewer than twelve eruptions of basalt may be counted which have taken place around the margin of the basin. But owing to the deep deposit of tufa, as well as to the thick sward of grass by which they are much concealed, their relation to the rocks through which they have been protruded is peculiarly obscure. Indeed, a suspicion naturally arises, that some basalt knolls may be completely buried beneath dense beds of tufa, and hence, that the number of them is inappreciable.

On the east of the basin, being at the very brim of it, a point

of eruption may be traced, where a stream of lava has flowed into the valley for a distance of about a quarter of a league. There is also reason to suspect, that about a quarter of a league nearly south of this point, being to the south-east of the basin, another eruption of basalt still more considerable took place, from which a flow has arisen, that has rather bent its course eastward to the exterior of the valley, as there is here a considerable elevation of the ground, which, from its narrowness, as well as from its being covered over with basalt blocks, points to this circumstance. But the ground is so concealed with a thick sward, as well as with tufaceous debris, that this can be only considered as a plausible conjecture.

On the south of the basin, there are at least three basalt knolls, and if we add to these a high cone of clay-slate, capped with the same volcanic substance lying about a mile due south, or nearly half way in the direction of the Hoch Simmer, the number may be rated at four.

On the west of the basin the eruptions rather indicate a more recent basalt, particularly near Volkesfeld, and at Wabern. But on the north west of the basin, there are indications, though ambiguous ones, of an older breaking out.

On the north of the basin, the precise limits of which are to an extraordinary degree obscure, owing to thick beds of tufa, two or three eruptions of basalt, of apparently an ancient date, may be detected.

This is the result of my attempts to determine the sites where the presence of early basalt is manifested. The investigation, for the causes assigned, is in many respects an imperfect one.

Whether any regular crater was formed during these eruptions, there is no unequivocal proof to show. There are some indications which would lead me to suspect, and nothing more, that externally to the south-east walls of the basin of Rieden, a considerable one might have very early broken out, of which faint traces, consisting of the hollowed out segment of a large circle marked by layers of a rather darkish-coloured tufa mixed with cinereous particles, are discoverable. On this assumption we might reasonably anticipate, that, owing to the extraordinary and varied convulsions which befel this particular site during

the commencement of the tertiary epoch, a loose cinereous fabric could scarcely avoid demolition.

The basalt erupted has almost an uniform character. It here shows its usual greyish black colour, and is particularly hard. It contains in its composition a larger proportion than common of felspar, which might indeed be expected from its having immediately supervened to trachytic eruptions. There are also thinly disseminated in its base, which is homogeneous, small crystals of augite or pyroxene, by which it is entitled, agreeably to Brongniart's nomenclature, to the name of *Basanite*.

In one or two places only could I detect any vitrification in the rock. I observed small portions of it in this state on the north of the valley, and I collected a few scorified fragments as well as cinders from the upper strata of the interior of the basin of Rieden; the presence of the latter giving fresh grounds for the suspicion, that one or perhaps more craters might have broken out on its margin.

These basaltic eruptions, which can only be traced to the exterior of the basin, appear in some places to have caused considerable disturbance to its tufaceous contents. This is shown more particularly on the east side, where the pre-existing strata appear in many spots greatly shattered. That most of the tufa was in a soft state when the basalt was protruded, is evinced in some of the strata which contain imbedded in them cinereous portions of basalt slag, the largest of which are about the dimensions of an inch and a half. And where the same tufa is found in junction with the basalt, it appears so altered by the heated mass, as to vie with the hardness of trachytic porphyry. It is of a yellowish brown colour, varied with specks of the same tint of a lighter shade. The cinders contained in it have been by the power of heat so firmly agglutinated in the mass, as to almost appear a part of its substance; their line of circumscription becoming proportionally indistinct.

But the greatest change induced in the valley of Rieden remains yet to be explained. Later invasions of volcanic rocks, which were protruded from the very brim of the basin, had caused in many places such an addition to the height of the

walls, that the original drain from the south-east was now resisted by the intervention of a far loftier bulwark, over which any new overflow would fail in effecting its escape. The more elevated point of the Gansehall, which now boasts a height of 1848 Rhenish feet, must have aided in producing this result.

The first consequence of this change was, that an increased capacity was given to the basin, which from this time would begin to be filled with tufa resulting from the wearing away of its marginal rocks of basalt. The decomposed substance induced, which was of a dark colour, readily became mixed with the disintegrated materials of pre-existing and lighter-coloured tufaceous strata, and hence the varied character of many of the upper beds of the tufa of Rieden, particularly on the south side. They are of a soft, loose consistence, of a very dark colour when first quarried, but growing paler after long exposure to the sun.

A second consequence would be, that any future overflow would be compelled to effect its discharge through some new formed channel. From this time, then, may be dated the commencement of a new ravine for the drainage of the valley on the south-west of the basin, by which its overflows were conducted into the ancient stream of the Nette.

Subsequently to this event, the basin of Rieden was doomed to experience no farther volcanic eruption. Little, therefore, remains to be recorded of its future history, except the appearances resulting from the gradually deepening corrosion of its new channel of drainage.

In the course of a period which admits of no calculation when attempted to be measured by historical annals, long corrosions have reduced the newer channel of drainage to its present depth. It is, notwithstanding, still so narrow, indicative, perhaps, of its comparatively recent origin, that it would be no difficult task to form an artificial barrier to the drainage of the valley, to lay the present unsightly village under water, and to convert the more elevated cones into a beautiful archipelago of wooded islets.

Coincident also with the drainage of this basin was the removal of its beds of tufa, and the exposure of its trachytic cones once submerged under a deep accumulation of boiling mud. Accordingly, in ascending a high point west of the

Gansehalls, we look down upon a most irregular fissure or cavity, exhibiting in its outline a thousand different forms, in the interior of which numberless volcanic peaks start up, the sides of which, as well as of the walls of the basin, are in general lined with thick strata of tufa, the remains of an upfilling mass, which in the course of ages has been removed by disintegrating causes, and carried away by mountain torrents.

SECTION VI.—GENERAL REMARKS ON MUD VOLCANOS, SUGGESTED BY THE PHENOMENA OF THE BASIN OF RIEDEN.

In the three foregoing sections, we were required to conceive of the focus of a volcano, in which many minute and light pulverulent particles were elaborated, being called into action beneath an abyss half filled with incandescent lava, into which flowed various mountain streams. Under these circumstances it was added, that the superambient waters of the lake would intercept all the light particles of volcanic matter, which would otherwise have been projected high in the air, and widely dispersed over a large surface of territory, causing them to be mingled with water of a temperature highly elevated from its contact with incandescent lava ;—and that the mixture would then assume the consistence and character of a liquid mud.

The contemplation of a process like this, which, in a basin like that of Rieden, some miles in extent, has caused every space unoccupied by its protruded trachytes to be filled with a tufaceous mud, becomes not a little astounding ; our surprise being increased by the fact, that overflows from this deposit have filled adjoining vallies, and have even been diffused among the lacustrine waters of the basin of Neuwied. A question, then, which irresistibly intrudes itself, is,—have the causes in operation been really equal to the accumulation of these suspended earthy materials to such an amount ?

To this question an unhesitating answer may be given. So infinitely great is the amount of light pulverulent particles which modern volcanos during their activity have given out, that, when carried into the atmosphere, they have formed clouds so thick as to deprive extended regions of the light of day. For instance, in the year 1794, during an eruption of Vesuvius, Calabria had its atmosphere entirely obscured by the light cinereous particles which were propelled ;—again, on the

authority of Procopius, we are assured, that at a prior period, namely, in the year 472, so vast was the abundance of them, that they were diffused over remote countries, as far even as Constantinople. Let us then suppose that a similar elaboration of the light pumiceous or pulverulent particles of trachyte should have taken place from a volcanic vent which a deep lake had submerged; and we have a cause perfectly adequate to explain why, in their interception by lacustrine waters, and in their diffusion through them, a basin so capacious as that of Rieden should have been filled even to an overflow with tufaceous mud or moya.

With respect to the sources of the immense supply of water, in which a corresponding proportion of pulverulent materials has been suspended, there is comparatively little difficulty. It is evident that the water might exist under more circumstances than one.

In the first place, craters induced by volcanic agency often become gradually filled with water; and as the more ancient ones must have been the result of an energy exceeding what is manifested at the present day, lacustrine sites, commensurate with the greater extent of the basins induced, would, in the course of new eruptions, be filled with boiling mud.

In the second place, the source of aqueous supply may often be traced to pre-existing inland lakes, beneath the beds of which volcanos have burst forth, accompanied with the formation of craters and the ejection of pulverulent matter, which has consequently been diffused through the waters. This fact is illustrated in the numerous basins necessarily coincident with recently elevated lands, few of which would possess incipient outlets for their drainage; and hence the many mountain masses of tufa which may be referred to accumulations of volcanic mud unable to effect their escape, and which have consequently been diffused over nearly the whole extent of a lake, frequently in almost incredible depths. Striking illustrations of this fact may be found in the Mont Dor and the Cantal of the south of France, or in the vicinity of Albano in Italy. The immense beds of tufa at the Bay of Naples had their origin in circumstances perfectly analogous, namely, in the trachytic volcano

of Monte Somma first bursting forth amidst the waters of an estuary rather than of an inland lake.

Lastly, the source of aqueous supply may be often traced to large fissures formed in the interior of a mountain, either during the elevations and subsidencies of volcanic convulsions, or from the long corrosion of percolating waters. Thus, it is recorded of Vesuvius, that in the year 1751 a stream of water so immense issued from a fissure, into which rains or snow had percolated, as to acquire the name of the *Nilo d'Acqua*. And when volcanic mountains attain an elevation approaching the line of perpetual congelation, accumulated masses of snow melted by volcanic heat have been known to find their way into caverns so vast, as to form spacious subterranean lakes, compatible with vegetable as well as animal existence. In the year 1698, a lake in the interior of the mountain of Carquarazo, which, during volcanic paroxysms, had been filled with pulverulent matter, burst and covered over a space of country to the extent of eighteen square leagues with a boiling mud, in which fish were inhumed. The substance thus ejected, which first in South America received the name of *Moya*, is said to have resembled in consistence *bouillie*, and to have hardened upon cooling.

These observations I have deemed fit to make from their being calculated to show, that the phenomena of mud deposits are less to be regarded as belonging to the rarer incidents of ancient or recent eruptions, than to those which are among the most familiar ones in volcanic history. In the district of the Laacher-see many other extinct volcanoes of a similar character might be cited, among which I might mention the basins of Fusel and Wehr, or the craters of the Lummerfeld and the Humrichs.

M. Steininger of Treves has been the first who named the tufa of the vicinity of Bell, as well as of other places, a mud eruption. But he has neither been able to point out its true origin, nor to see its necessary dependence upon some contiguous crater lake. He has been content with referring its elaboration to the volcanic focus itself, conceiving that it had the self-same origin as an incandescent lava stream. This geologist has been opposed by many writers, among whom might be cited Dr Daubeny. (*Daubeny on Volcanos*, p. 60, &c.) One of the objections of this last named gentleman is from "the want of connection which the tufa has with neighbouring craters."

But Dr Daubeny is not the only geologist who has failed in seeing this connection. Amidst the volumes which have been written upon the volcanos of the Lower Rheinland, it is not a little remarkable, that the information which has been hitherto collected of the basin of Rieden, of its immense trachytic eruptions, or of its accumulations of tufa, is so scanty, that the whole of it might be contained within the compass of half a page. It is surely time, then, that its pre-eminence over almost every volcano in the Lower Rheinland should be duly appreciated.

ART. XIX.—*Notice respecting certain Vibrations of Heated Metals.*

ABOUT a year ago, Mr Arthur Trevelyan communicated to the Royal Society of Edinburgh an account of some sounds accompanied by vibrations, observed by him during the cooling of several metals placed in contact with lead. This fact had been known to Mr Trevelyan since February 1829, though he had neither announced it nor undertaken to analyze the subject, until near the time of that communication. Subsequently, he communicated his experiments to Mr Faraday, who delivered a lecture upon them about six months since at the Royal Institution, some account of which has since been published, and, with the exception of a few experiments made by Professor Leslie, no notice has been published of any farther inquiries.

The subject appears, however, sufficiently interesting still to excite the investigation of those who may be disposed to form any opinion, strictly supported by experiment, as to the real cause of the phenomena, which has yet hardly been attempted. We propose, therefore, merely to announce as an interesting fact, the phenomenon observed by Mr Trevelyan, beyond the establishment of which his experiments do not appear to have much extended.

When a bar of iron or copper, such as A, Plate I. Fig. 5, having on its lower side a flat ridge from which the surface is bevelled upwards on either hand, is placed, after being subjected to a moderate heat, upon the mass of lead B, and inclined as represented in the figure, a vibration at right angles to the

axis of the bar immediately commences, during the continuance of which the bar is alternately poised on each solid angle formed by the ridge, and falls back in consequence of the displacement of its centre of gravity. This vibration goes on until the temperature of the bar and block become equalized or nearly so.

If the surface of the bar and block be both even, the sound produced by the successive impacts is not a musical note. If, however, there be an indentation or groove in either one or other, the pitch rises to that of an audible note, and in some cases the vibrations are sufficiently rapid to produce a pretty high note, which may also be done by pressing gently the bar upon the block. Mr Faraday first pointed out accurately the nature of these sounds, which, however, Mr Trevelyan appears still to attribute to a hypothetical current of air passing through the groove, of which he assumes the existence, and which, even supposing it proved, can be shown by the simplest experiments to have no influence whatever in the production of the sounds.

These phenomena are not confined to iron and copper as the hot metals, or to lead as a block; zinc, brass, and other metals may be substituted for the former, and tin answers pretty well instead of the latter. Hot silver vibrates upon cold iron. According to Mr Faraday, this fact has been long known to silversmiths. The form of the bars is of little consequence, provided there be two distinct points between which the vibrations can take place, or which act as the solid angles of the ridge in Mr Trevelyan's bar. Mr Robison of Edinburgh obtained highly musical notes, by placing upon cold lead a hot poker, a coach spring, and a silver spoon.

When we add that a cold *bar* of lead placed on a heated *block* of a hard metal vibrates, and that in no instance have these vibrations been satisfactorily observed except when metallic substances were employed, we shall have stated nearly all that can be gathered from Mr Trevelyan's experiments.

Professor Leslie made some experiments on the subject at an early period, and found that the vibrations continued in the exhausted receiver of an air-pump. He suggested, in explanation of the phenomena, that the impulse at each vibration was given by the expansion of the cold metal during the evanescent contact of the hot one, but he did not enter into any

examination of the notable differences of action exhibited by different metals. His views were adopted by Mr Trevelyan, and afterwards with little modification by Mr Faraday, both of whom attempted to reconcile them to the observed phenomena, with what success, however, we shall not at present inquire, as we wish to avoid a premature discussion of the theoretical bearings of the question.

ART. XX.—*Notice on the propriety of giving Representatives to the principal Scientific and Literary Institutions in London.*

It has been suggested by some gentlemen in London of high scientific eminence, that two or more representatives should be given under the new Reform Bill to the following Scientific and Literary Institutions in the Metropolis, viz.

The Royal Society,
The Royal Society of Literature,
The Geological Society,
The Astronomical Society,
The Royal Academy,
The Antiquarian Society,

The College of Physicians,
The College of Surgeons,
The Geographical Society,
The Royal Asiatic Society,
The Linnean Society, &c.

The necessity of representing in a reformed Parliament the *intelligence* as well as the property of the country, has been admitted in the most express terms by Earl Grey and the rest of the Cabinet; and the safety of such a measure is not likely to be denied by those who have expressed so much anxiety respecting the character of the constituency of the House of Commons.

In former times the intelligence of the age was concentrated in our universities; but during the rapid progress of knowledge new institutions have sprung up, and by their vast and increasing influence the intellectual strength of the nation has been developed and organized. The parent institutions are no longer the sole depositaries of science; and much of the genius of the age now adorns those gifted associations which owe their existence and their permanence to individual liberality.

The modern establishments for the promotion of literature, science, and the fine arts, bear the same relation to the vene-

nable institutions of our colleges, as the great manufacturing towns bear to some of our ancient though still flourishing burghs ; and the same arguments which are employed for giving representatives to the seats of our national industry may be used, for giving them to the chartered associations of our literature and science.

A proposal was made in the House of Lords by an influential Scottish nobleman (the Earl of Haddington) to give one representative to the University of Edinburgh. The difficulty of finding a sufficiently extended constituency was urged as an objection to this very reasonable suggestion ; but the difficulty might be completely removed by uniting to the University the Royal Society of Edinburgh and the Society of Scottish Antiquaries. In like manner the Royal Irish Academy might be united to the constituency of the University of Dublin.

ART. XXI.—*Specimen of Logarithmic Tables printed with Different Coloured Inks on Various Coloured Papers.* By CHARLES BABBAGE, Esq., M. A., Lucasian Professor of Mathematics in the University of Cambridge, F. R. S. L. & E. In Twenty-One Volumes, one Copy only having been printed. London, 1831.

As we have had the advantage of seeing and examining the truly *unique* work of which the above is the title, we think our readers will be gratified with some account of it, as well as of the objects which its public-spirited author had in view in making so costly an experiment,—and of the general results which may be obtained by extending the inquiry.

The following is Mr Babbage's own account of the experiment, which he has prefixed in the form of a preface to his work :

“ The object of the experiment for which these volumes were printed, was to ascertain the colour of the inks and the tints of papers least fatiguing to the eye. For that purpose one hundred and forty differently coloured papers were chosen, and ten different colours of ink were employed.

“ With respect to the papers, it was not easy to procure them in sufficient variety and in proper sizes ; many have been cho-

sen which obviously would not succeed ; but as the additional expense was not great, it was esteemed desirable to print one on every different coloured paper which could be purchased in London.

“ It was found difficult to procure ink of a pure colour : ten varieties have been employed.

“ The work has been bound in twenty-one volumes ; the first twenty of these consist of ten sets of two volumes each. Each set contains about one hundred and forty differently coloured papers, on each leaf of which the same two pages of logarithms are printed. As far as possible the two volumes of each set ought to be identical, but owing to accidents in printing this is not always the case. On each separate coloured paper is a number written in ink. When these numbers are interrupted, it is to be understood that the papers corresponding to the intermediate numbers are wanting. The same number always indicates the same paper throughout all the volumes. The twenty-first volume contains specimens of the same printing in gold, silver, and copper, upon vellum and variously coloured papers. It rarely happened that more than two leaves of the same paper were printed upon, so that when one was spoiled, after they had been worked off, it was impossible to replace it : this accounts for the occasional deficiencies, and for the fact that very few duplicates of any paper remain.

“ The reason for having two sets of each differently coloured ink was, that comparisons might be instituted by laying open, side by side, the same table printed with the same ink, but on differently coloured paper. When comparisons between inks of different colours are made, the two required volumes may be laid open either at the same or at differently coloured papers.

“ From the result of some preliminary observations, the following points are suggested for inquiry :—

“ 1.—Generally, whether white paper is as good as lightly tinted papers.

“ 2.—Whether black ink is preferable to all other coloured inks.

“ 3.—Whether that colour both of paper and of ink, which is preferable in a strong light, is equally so in a weak light of the same kind.

" 4.—Whether those colours of paper and ink, which have been found most agreeable in day-light, will remain so when artificial light is used.

" 5.—Whether the colours of paper and ink which are least fatiguing, will not depend on peculiarities in the organs of sight of the individuals who make the judgment.

" 6.—Whether the state of general health of the observer may not affect the organ of sight, and render different colours agreeable in different states of health.

" 7.—Whether there are not some colours of ink, and some shades of colour of papers, which are most agreeable to the majority."

When Mr Babbage's machine is completed and ready to compute and print those important tables which are required for the purposes of navigation and astronomy, it will be an object of the very first consequence to determine the colour of the papers on which they should be printed. In order to prepare the way for such an inquiry, we shall proceed to make some observations on the *seven* points which Mr Babbage has suggested for consideration.

1. *Whether white paper is as good as lightly tinted papers.*

The general advantage of using the *blackest ink* and the *whitest paper* is, that the contrast of the black figures against the white ground is a maximum, and therefore the figures may be read with very faint illumination, at certain stages of the twilight, or at certain distances from an artificial light at which they would otherwise be illegible. This is a *positive* advantage, which must not be slightly rejected.

The general disadvantage of this maximum degree of contrast is, that the eye is sooner fatigued than when the figures are printed upon a less luminous ground. This fatigue arises from two causes,—*1st*, from the constant strain upon the muscles of the iris necessary to contract the pupil beyond the aperture through which the eye generally looks at ordinary objects; and *2d*, from the effects of undulations which are propagated over the whole retina whenever the eye is strongly impressed with a number of white and black lines strongly contrasted.

The disadvantage, however, now described, may be entirely removed by using a less degree of light, or by looking through any darkening medium, such as wire-gauze, &c. while we at the same time retain the advantages previously mentioned.

The general advantage of very *lightly tinted* papers is, that they are less injurious to the eye in strong lights, in so far as they reflect less light to the eye; for when they are very lightly tinted, their colour does not become a matter of consideration. Their disadvantage is, that the figures will not be so easily read in faint lights; and that whatever advantage may be derived from colour will be ultimately lost by its dilution, or its entire disappearance in the course of time. It becomes also a point of serious consideration how far the darkness and permanency of the ink may be affected by the action, whether quick or slow, of the colouring matter in the paper. It is well known that the finest and blackest ink becomes brownish when the paper has been bleached with the oxymuriatic acid.

2. *Whether black ink is preferable to all other coloured inks.*

Black ink is decidedly preferable, *optically* speaking, to all other coloured inks upon *white*, or *very slightly tinted* papers, because the contrast is a maximum. It remains, however, to be ascertained what colour of ink is least likely to change with time, or from the action of the colouring matter in the paper.

3. *Whether that colour both of paper and of ink, which is preferable in a strong light, is equally so in a weak light of the same kind.*

We are disposed to answer this question in the negative. Ink and paper of a powerful contrast are obviously the worst in a strong light, from the causes already mentioned; but in a weak light of the same kind they are clearly the best. If papers of a deeper colour are employed, the difference of effect in strong and weak lights will be still greater.

4. *Whether those colours of paper and ink, which have been found most agreeable in day-light, will remain so when artificial light is used.*

This query must certainly be answered in the negative. From

a very extensive series of experiments which will soon be published, we shall be able to show that the composition of the whitest artificial light is essentially different from that of day-light. We have now before us *bright green* media, which are *yellow*, others *orange*, others *pink*, and others *deep blood-red* by artificial light, whether it be that of a tallow candle, a wax candle, a lamp, or coal gas. We have *blue* media which are *pink* and *red* by artificial light. We have media considerably opaque, which become more transparent by artificial light; and various coloured substances which change both in the intensity and nature of their colour when exposed to artificial flames. Hence it is obvious, that colours which in day-light have particular advantages, in reference to vision, must either lose these advantages, or possess them in an inferior degree when seen by candle-light.

5. *Whether the colours of paper and ink which are least fatiguing, will not depend on peculiarities in the organs of sight of the individuals who make the judgment.*

A very great number of persons have various degrees of insensibility to particular colours. Hence it is manifest, that the colours which may be most pleasing or useful to the majority of observers, may be least so to those who have this defect. If a good red-coloured paper, for example, should be agreeable to some eyes, it would be an extremely faint yellow to eyes insensible to red light, and consequently, it would be unfit for such eyes, both from want of light and from want of colour.

6. *Whether the state of general health of the observer may not affect the organ of sight, and render different colours agreeable in different states of health.*

Nothing is more certain than that vision is greatly affected by the state of health, and particularly by the condition of the digestive organs. Feeble contrasts of ink and paper, which are sufficient to enable a person in perfect health to see distinctly and read easily, will not be sufficient when the eye is muddy or dim with an affection of the digestive organs, or when the pressure of the blood-vessels on the retina is producing a continued phosphorescent undulation over the whole of that membrane.

In such a state of the eye the strongest contrast of black ink and white paper is absolutely requisite.

There are also certain states of disease in which the eye becomes insensible to particular colours. In such cases the effects of different coloured papers will vary as described under § 5.

7. *Whether there are not some colours of ink, and some shades of colour of papers, which are most agreeable to the majority.*

There will undoubtedly be some colours of ink and some shades of paper which are *most agreeable* to the majority of observers, but they may not be the *most useful*. If the two colours are the *accidental* or *harmonic* colours, they will undoubtedly be the *most agreeable* to the eye, but not the most effective, owing to the want of sufficient contrast.

From these observations we are disposed to conclude in general, that the *blackest ink on the whitest paper* ought to be adopted, even if it should be found that great advantages arise from coloured inks and coloured papers, or from black ink and coloured papers. The ground of this conclusion is, that by looking through slightly-coloured media, we may give to the white paper any tint we desire, without depositing a poison at the root of the ink; and in cases of feeble illumination we can remove our coloured medium, and obtain all the advantages of the white ground when it is really needed.

The following are the principles upon which the colours of such media should be chosen, and upon which the colours of papers should be selected, if they shall be thought advisable.

The various tints which occur in nature, or which are obtained artificially from mineral and vegetable products, have the most singular variety of composition. An *orange* colour, for example, may consist of rays of every degree of refrangibility from the extreme violet to the extreme red, its orange colour arising from the absorption of many of the blue and green rays. Such an orange is highly unfitted for vision, from its great range of refrangibility. Another orange colour may consist of the red and least refrangible yellow, so as to have a narrow range of refrangibility, and this orange is highly favourable to vision. A *bluish-green*, such as that of sulphate of copper, which absorbs all the extreme red and the extreme violet rays, is also

favourable to vision, as it consists of rays close to one another in the spectrum. The finest *yellow*s are those produced by *orpiment* and the *carbazonic acid*, which have the singular property of cutting off or absorbing one-half of the spectrum, and that the least luminous one, viz. the violet and almost all the blue rays. They transmit, therefore, a great body of light, and consist of rays differing slightly in refrangibility. All *pink* colours are bad, as they absorb the middle, and leave the extreme rays of the spectrum; and the same character attaches to all *blue*s, such as those of glass of *cobalt*, which become redder by increase of thickness. The best blue is the *ammonio-sulphate of copper*, and the *carbonate of copper* is also good. All those *green*s are bad which absorb a space between the red and green of the spectrum. This property, however, is not possessed by the green glass commonly used.

ART. XXII.—*Remarks on an Introductory Lecture delivered in King's College, London, October 11th 1831.* By J. F. DANIELL, F. R. S., Professor of Chemistry in King's College, London.

“WHO can tell us any thing of the sulpho-salts? who will explain to us the laws of isomorphism? Nay, who amongst us has ever verified Thenard's experiments on the oxygenated acids? Oersted's and Berzelius's on the radicals of the earths? Balard's and Serrullas's on the combinations of brome? and a hundred other splendid trains of research in that fascinating science of chemistry.” These questions which, according to Mr Daniell, “have never, possibly, been answered, for the simple reason that the answers to them are so obvious that no one has thought it worth while to make of them a formal display or a boast,” form the main topic treated in the introductory lecture read before the patrons and founders of King's College, London. We think it neither unnecessary nor too late, after a lapse of nearly *two** years, to reply to these questions; and, interested as we are in the science of chemistry, we should rejoice in seeing them successfully answered. We do not think

* The date of Sir John Herschel's questions is February 1830.

Professor Daniell has succeeded, but we shall insert his entire reply, that our readers may have the whole case before them.

"We are asked, 'Who among us has ever verified Balard's and Serrullas's experiments upon brome?' Most simple is the reply—In the courses of lectures given in the Royal Institution, and in the London University, of which I can speak from personal knowledge, bromine and its compounds are as regularly introduced and illustrated by experiments, as chlorine or iodine. It ought also to have been known to those who so unsparingly censure the ignorance of others, that the professor of chemistry in the University of Oxford has not only verified the experiments referred to, but has extended the discovery of the new elementary substance to the brine springs, and other mineral waters, of this country."—P. 9.

Now, in this and in some of the succeeding paragraphs, Mr Daniell assumes a position which, in the present state of chemical science, cannot be granted him. To prepare brome from bittern, and for our own amusement, or the instruction of others, to form the acids and other compounds of brome, is not to *verify* Balard's and Serrullas's experiments. Mr Daniell knows the care and accuracy now necessary to verify the experiments of others; and yet, according to his mode of reasoning, mere *repetition* is equivalent to *verification*. The chemistry of England would have been at a low ebb indeed, had its cultivators satisfied themselves by merely incorporating the discoveries of others into their class experiments, and deemed such repetition all the verification they required. But they have been *verified* at Oxford. Dr Daubeny himself claims no such merit. The little that has been done for brome in this country, the detecting it in our mineral springs, is due to the Oxford Professor, and to him we are so far grateful; yet still the experiments referred to have never been verified in England. We do not think any such verification *now* necessary, nor probably does the distinguished individual who asked the question; but should it not be considered a matter of reproach to English chemists, that a new substance, accessible to all, should be discovered, examined, and described, and that, while the chemists of every other scientific people were adding to the list of its properties

and compounds, they alone should not have contributed a single fact to its history?

“The question is also asked, ‘Who among us has ever verified Oersted’s and Berzelius’s experiments on the radicals of the earths?’ And can it really have been supposed for a moment, that the professors and directors of that laboratory, from which emanated the first glorious train of research which ended in the decomposition of the alkalies and earths, have become so inactive and inefficient as not to have the desire, or the ability, to verify experiments, which are the mere loads and veins of that mine which was opened under their own inspection? Has Davy indeed left no mantle behind him? no disciples worthy of their master? Well may we lament for these degenerate days!—if we do not rather grieve that such things should be assumed, without due inquiry, for purposes of de-traction. I can, however, vouch for it, that at the Royal Institution these objectors might have witnessed the production of magnesium, aluminum, and silicon, with much less trouble than it cost them to transcribe these questions: and to the merest tyro of both the schools which I have named, I refer them for the manner in which the processes are explained.” P. 9.

None can have a higher respect for the Royal Institution than we. The true lover of chemical science must reverence the birth-place of its finest discoveries. We are not singular also in placing him on whom the mantle of Davy has rested at the head of English chemists, and we believe that the progress of chemistry is more closely followed *experimentally* in the Royal Institution than in any other school in Britain. But on these repetitions of the experiments of others, or *verifications*, as Mr Daniell calls them, English chemistry must not rest. The scientific fame of England among foreigners and with posterity will rest on her works of science, and the contents of her journals. It would have been to the purpose could Mr Daniell have shown in any such work a single new observation by any British chemist on the radicals of the earths alluded to.

“Again, it is inquired, ‘Who has ever verified Thenard’s experiments upon the oxygenated acids?’ Now this question may not, probably, have been inappropriate at the time when

it was first distrustfully proposed:—but, long before it was prominently arranged with others, and extracted from a position in which it is allowed it was ‘unlikely to meet with general attention,’ the distinguished author of the experiments had published to the world, that he had been mistaken in the conclusions which he had originally drawn from them. The appearance of oxygenated acids, he proved, had been conferred by the presence of a new compound of oxygen and hydrogen, in which the quantity of the former element was double the proportion in which it exists in water; and upon which he bestowed the name of deutoxide of hydrogen. And yet the chemists of this country are reproached with not having verified these abandoned conclusions! Does not this resemble the conduct of men, who wilfully close their eyes, and perversely exclaim, that all the world is enveloped in gross Egyptian darkness? The discovery of the deutoxide of hydrogen—the legitimate result of M. Thenard’s laborious experiments—has been verified in the Royal Institution; and if the new compound be not ordinarily exhibited in lectures, as a class experiment, the expense, not the difficulty, of its preparation prevents the illustration.”—P. 10.

This question, Mr Daniell allows, might be appropriate when first asked. We hold the *inquiry* not so necessary now, but the *question* is still appropriate. Thenard has altered his views; but to whom are we indebted for this alteration? Did British chemists repeat or verify his first experiments?—Has a British chemist ever published a paper on the subject?—if not, for all that England has done, Thenard’s error might still have found a place in our text books—and for all they are yet willing to do the French chemist may still be in error.

“ ‘Who will explain to us the laws of isomorphism?’ proceeds the objector. If by this a doubt is meant to be expressed, whether the phenomena of isomorphism, as developed by their discoverer, Professor Mitscherlich, are duly known and appreciated in this country, I again appeal to the classes of our schools: but if we are accused of ignorance of the general laws which connect these facts together, it is an ignorance which we share with the natural philosophers of other countries; and greatly do I doubt whether much more of observa-

tion must not precede any useful attempt to theorize upon this interesting subject. Perhaps I may even be allowed to say, that some hesitation is excusable in the adoption of such generalizations of the subject as it has been already attempted to extend to mineralogy, when we observe a class of *silicates* distinguished, in which the *silica* is wholly replaced by *alumina*. That England, moreover, is not at the present moment destitute of men who are competent to reason, if not precipitately, closely and powerfully upon this subject, has lately been evinced in a paper which adorns a late number of one of our scientific journals.*—P. 12.

One would almost be led to doubt the candour of Professor Daniell when we find him appealing thus triumphantly to the schools. We will not affirm that, when two years ago the question was asked, the doctrines of isomorphism were not known in our schools; but it would be a difficult task to prove that they were taught. We believe, in fact, that when the article on Sound in the *Encyc. Metrop.* was written, there was not a word published on the subject in our language. Even now Dr Daubeny considers it so little known that he has thought it necessary to enter upon the subject at some length in his recent work. "It has been remarked," he says, "as a proof of the low state of science in this country, that the laws of isomorphism are but just beginning to attract notice here, whilst they have for several years engrossed the attention of continental philosophers. And although, since the above statement was made, a brief sketch of these researches has been given by Dr Turner in the new edition of his *System of Chemistry*, (1831,) drawn up with his usual clearness, it may not be uninteresting to have a more detailed statement of these new views exhibited in an English dress for the sake of those who may wish to prosecute the subject further."†

And what does Mr Brooke's paper prove. That at the time Sir John Herschel wrote little or nothing was known on isomorphism, for the doubts Mr Brooke threw out in September,

* On Isomorphism. By H. J. Brooke, Esq. F. R. S., &c. *Phil. Mag.* Sept. 1831.

† Introduction to the Atomic Theory, p. 68.

he might equally have stated before had he known the subject as well. And even Mr Brooke does not profess to add any new facts to our former knowledge on the subject, nor, we believe, has any British chemist, by his experimental researches, thrown any additional light on this interesting subject. Such researches are akin to many of Professor Daniell's own elegant inquiries. Let him enter as an ardent labourer into this field, and he will do more for the maintenance of British chemistry, than by a host of elaborate lectures in its defence.

"But foremost stands the question, 'Who can tell us any thing of the sulpho-salts?' To this, I trust, you will excuse me if I reply a little more at length. Highly distinguished as I have been, and far, I fear, above my merits, by my appointment to the chair of Chemistry in this College, it became my first duty to adopt such a plan of instruction, as, upon mature reflection, I might feel convinced would lead the student forward by an easy and safe advance. I felt myself called upon to review with care, the different arrangements of the science which had been made by the most distinguished professors, for the purpose of adopting such as might most facilitate the progress, and assist the memory, of my pupils. In the course of this review, that which has most fixed my attention and called for most consideration is the new arrangement and the new nomenclature proposed by the distinguished Swedish chemist, Berzelius, of which the division of *sulpho-salts* forms an important part; and the result of my deliberation has been to reject a change, which, by unnecessarily increasing a class of compounds, already too extensive for convenience, must greatly add to the difficulties of a new beginner. This new arrangement, and this new nomenclature, have not hitherto been adopted by British chemists; with one very recent exception: and I think that, notwithstanding the deservedly high reputation of the author, they have done wisely to suspend their judgment upon them. At the same time the exception of so distinguished a chemist as Dr Thomson, although I differ from him in opinion, is amply sufficient to enforce my argument that it has been hastily and inconsiderately concluded that British chemists *could* not tell us any thing of the *sulpho-salts*; while the hesitation of the majority, I believe, proves any thing but that they do not understand the subject."—P. 12, 13.

We do not insert the rest of the reply to this last question, because it is unnecessary here to enter into the discussion of the sulpho-salts. But what is the substance of the passage quoted? Sir John Herschel asked the question, "Who will tell us any thing of the sulpho-salts?" Professor Daniell answers, When I was appointed to the chair of chemistry in the London University, *upwards of a year after* the question was asked, I thought it my duty to *examine the subject* of the sulpho-salts, and to reject the change they would introduce. Dr Thomson has adopted the change six months later still; and, therefore, it has been hastily and inconsiderately concluded, that British chemists could not tell us any thing of the sulpho-salts!! This reasoning may be conclusive enough, though we cannot follow it.

And Professor Daniell rejects the change introduced by the *sulpho-salts*, because, by *unnecessarily increasing a class of compounds already too extensive for convenience*, it must greatly add to the difficulties of a beginner. We marvel that after such reasons assigned, chemists should think of searching for new compounds,—those we possess being already too numerous for *convenience*. Why *increase unnecessarily* the number of chemical substances, either simple or compound? You are only adding to the difficulties of the new beginner!

We make but one extract more. "In collecting together the 'scattered' evidence of the fall of chemistry, the accuser has found one more specific charge from a different quarter. Some of our most eminent chemists, it appears, have successfully directed their attention to other branches of natural philosophy, and have pre-eminently advanced the science of electro-magnetism: they are therefore arraigned for a misdirection of their talents; and are held responsible for not having decomposed nitrogen, and insulated the suspected fluorine. To this it is indeed difficult to return a serious answer. What, if we were to mete by the same narrow measure! What might not we say to the profound mathematician who quits the calculus for chemistry, not to perform the labour of a successful cultivator of the field, but to undertake the inglorious task of depreciating the exertions of others? But I refrain."—P. 16.

We have already expressed our high regard for Mr Faraday,

and, though we admire all he has done, as pure chemists we grieve he should seem to have deserted our favourite science. We say this not by way of blame, for every philosopher has a right to follow that path into which circumstances and inclination lead him. Who shall bind any man to a single pursuit? or hinder him from making excursions into other fields of science than that he has hitherto illustrated? And yet who shall say we are not to regret when we find a philosopher at any time of life deserting that walk of science in which he seemed best fitted to excel. Professor Daniell, we think, will in this agree with us;—why, then, has he adverted to the “profound mathematician * quitting the calculus for chemistry, not to perform the labour of a *successful cultivator* of the field, but to undertake the inglorious task of depreciating the exertions of others?” We might ask Mr Daniell what he has discovered during his successful cultivation of his proper field, of greater importance than the hyposulphuric acid and its salts?—but we refrain.

What advantage, then, has the chemistry of England derived from the introductory lecture of Mr Daniell. Simply, none. Or has he applied any salve that can heal the national vanity, hurt, as some say, by the interrogations of Sir John Herschel? We can perceive none. In so far as the dispute is one of words, it matters not which of the two be right. But which of them takes the view most justified by the researches of British chemists? No British chemist has published any additions to our knowledge of brome, of the radicals of the earths, of Thenard's *liqueurs oxygénés*, of isomorphism, or the sulpho-salts. Of the two latter, indeed, the knowledge has only begun to spread in this country. Does British chemistry, then, stand any higher in the judgment of impartial men since Mr Daniell's paper was published? It is impossible. Grant all he has asked for, and it will merely be that chemistry is standing still. And is this no reproach?

But these subjects are taught in the metropolitan schools of science. We hope and believe they are, and on this we found our prospects of the future. But it were a second and a worse blot on the chemists of this country were they, besides not ad-

* When going to press, we have learned that Professor Daniell does not mean this passage to apply to Sir John Herschel, and are happy in being able to correct a mistake into which others besides ourselves have fallen.

ding to, actually ~~not to teach the discoveries of foreigners~~. No such charge has been made against them, and we trust it is unnecessary.

Have the questions in the *Treatise on Sound* done harm, then, to the cause of science in England? Quite the reverse. Not to travel out of our own department, they have done good to the cause of chemistry. The terms isomorphism and sulpho-salts have reached the ears of many during this discussion for the first time; and we speak advisedly in saying, that well read chemists were awakened by it to the necessity of making themselves acquainted with these important topics. The object of the writer in asking the questions was obviously, as we understood it at least, to draw public attention to the little we were doing for the most important of sciences, while foreign chemists were daily enlarging its boundaries. This end has been partly accomplished; and the lectures to which Professor Daniell so triumphantly points as a full reply to the charges he combats, are probably owing to the public and authoritative manner in which these very questions were propounded.

Professor Daniell has passed over the "hundred other splendid trains of research," with which the questions conclude, as containing nothing tangible, and as not therefore calling for any reply. And are there not many such to which no British chemist has directed his attention? We ask not apparent impossibilities. We do not "hold them responsible for not decomposing nitrogen or insulating the suspected fluorine." But what foreign chemists have done and are doing we may do. Who has deduced from his own analysis one single general formula, representing the composition of any large class of mineral substances? Who has *rigorously* verified any number of the atomic weights of Berzelius? Has any one even repeated, we do not say verified, Zeise's researches on xanthogen and its compounds,—or his later investigations on the singular compound salts formed by the chloride and double chlorides of platinum with alcohol? Dr Thomson has adopted, but who has examined experimentally, the opinions of Bonsdorf regarding the double chlorides? And, except Edmund Davy's paper on the fulminates, which we know not why the last published *System of Inorganic Chemistry* follows in preference to the repeated researches of Liebig, who has made any experi-

ments on the interesting acids of cyanogen? But we might, indeed, pass over all the new conquests of chemical science, and ask which of them has England achieved. We do this, however, from no wish to depreciate our native country or native genius,—but rather to stir up a generous rivalry among the cultivators of this engrossing science. Better that we should spy out our own failings, even though a little too narrowly, than that other and foreign observers should discover and taunt us with our deficiencies. For ourselves, we are certain that British science cannot decline *when* the national mind has once been sufficiently awakened, for there are among us minds and means enough to raise it to as high a pitch of elevation as it has ever hitherto attained. It needs only that all who love it, instead of dividing their strength, so as to neutralize the efforts of one another, should unite their exertions in the same great cause, and they must prevail.

J.

ART. XXIII.—*An Introduction to the Atomic Theory, comprising a Sketch of the Opinions entertained by the most distinguished Ancient and Modern Philosophers with respect to the Constitution of Matter.* By CHARLES DAUBENY, M. D. F. R. S. Professor of Chemistry in the University of Oxford.

THIS work has appeared at a very seasonable time. The atomic theory of Dalton must soon undergo some modifications, and a book like that of Dr Daubeny was called for to show us the point at which we have arrived. It is the only work, in our language at least, which combines together, compares and distinguishes the opinions and results of ancient and modern philosophers. Too much space, perhaps, is occupied in discussing the infinite divisibility of matter. The atomic theory implying the existence of ultimate particles, derives its value from its applicability to the resolution of phenomena. Were it not so applicable, all the corroborations so ably and elaborately brought forward by Dr Daubeny would not gain it credence for a single day. This discussion, however, has not been introduced to the exclusion of better matter. We believe nothing has been omitted to which the author had access,

and perhaps others may only value the book the more highly that it contains a detailed exposition of almost every thing that can be advanced against the theoretical doctrine of the infinite divisibility of matter.

The work is divided into four chapters. The first comprises a sketch of the opinions entertained respecting the constitution of matter before the laws of definite proportions were discovered. The second, of the modern discoveries respecting definite proportions which seem to prove the existence of ultimate atoms. The third, the applications of which the laws of definite proportions are susceptible; and the fourth, the collateral arguments in favour of the existence of indivisible particles; and an inquiry as to how far the doctrine of definite proportions may have been anticipated by the ancients.

Under these several heads, Dr Daubeny has brought together a mass of interesting and important information, to which we can refer our physical as well as our chemical readers, not only with the prospect of entertainment but of much instruction. The opinions of the Indians, of Epicurus, of Anaxagoras, of Empedocles, Plato, Aristotle, and their followers, are all stated and compared with the modern theories and researches of Boscovich, Newton, Descartes, Leibnitz, Kant, Richter, Higgins, Dalton, Prout and Mitscherlich. The most interesting portions to us have been those regarding isomorphism and isomerism. Our limits permit us only to advert to the latter point, on which we request the attention of our readers to the following extract:—

“Professor Berzelius, in a paper published in the *Transactions of the Swedish Academy* in 1830, has enumerated several other examples of the kind, distinguishing them by the name of *isomeric bodies*.

“The phosphoric acid is one of them; when exposed to a red heat for some time, it acquires new properties, coagulating albumen, and producing white instead of yellow precipitates with nitrate of silver.

“The tartaric acid is another case in point, Berzelius having discovered in certain kinds of tartar an acid differing in properties from, though agreeing in chemical constitution with, that

more commonly known. The cyanous and fulminic acids are instances still more remarkable.

“Such are the principal examples of the kind taken from inorganic nature ; but among organic bodies they would appear, from the researches of Dr Prout, to be much more numerous.

“Thus the sugar from the cane, and from the urine of diabetic patients, agrees as nearly in point of composition with the sugar of milk, manna, and gum arabic, as the several varieties of cane-sugar do with each other ; yet the first class of sugars yield oxalic, the second saccharic acid.

“Professor Stromeyer concludes, that this discrepancy arises from the dissimilar arrangement of the component atoms, and the different degrees of condensation they have undergone ; but it appears to me more probable, that the presence of a portion of some principle, occasionally even too minute to be detected by analysis, may have occasioned the development of new properties. Dr Prout is of opinion, that some foreign body, not of itself belonging to the animal or vegetable kingdoms, necessarily enters into the constitution of every substance capable of becoming assimilated, and constituting a part of any organic structure. Bodies containing this admixture he denominated *merorganized*, in order to express this supposed condition, implying that in passing into this state they become partly, or to a certain extent, organized.

“Now he accounts for the exceeding diversity of properties possessed by organic bodies, whose chemical composition is nearly identical, by the admixture of this small proportion of foreign matter, which by its presence infuses new properties into the mass, and prevents the particles from arranging themselves in their natural crystalline form.

“Thus starch is *merorganized* sugar, differing only from the latter by the presence of certain foreign matters, which effect a complete change in its characters.

“This curious view is rendered more intelligible by the important researches of Mr Herschel, detailed in his Bakerian lecture for 1824 ; in which he has shown, that the relations of a mass of matter, such as mercury, to electricity, may be even reversed by the presence of an almost infinitesimal quantity

of a substance, such as potassium, in an opposite electrical condition.

“ ‘That such minute proportions of extraneous matter,’ says Mr Herschel, ‘should be found capable of communicating sensible mechanical motions and properties of a definite character to the body they are mixed with, is perhaps one of the most extraordinary facts that has appeared in chemistry. When we see energies so intense exerted by the ordinary forms of matter, we may reasonably ask, what evidence we have for the imponderability of any of the powerful agents, to which so large a part of the activity of material bodies seems to belong?’ ”

The first account of *isomeric* bodies which appeared in our language was contained in this *Journal*, No. vii. N. S. p. 130. The definition of isomeric bodies there given is that “with alike chemical *composition* and *atomic weight* they possess unlike properties.” That we shall meet with many such bodies in organic nature is highly probable, but the sugars of Dr Prout are scarcely admissible into that class. They possess the same atomic constitution; but do they possess the same *atomic weight*? Till the latter is proved as satisfactorily as Dr Prout has proved the former, they cannot be considered as isomeric bodies. If we suppose one series of atoms to constitute cane-sugar, two series diabetic sugar, three series the sugar of milk, &c. we should have bodies possessing the same atomic constitution, but whose atomic weights would be as 1, 2, 3. In such a case they would not be isomeric bodies.

Mr Daubeny attributes the discovery of the paratartaric acid (the acid of the Vosges *) to Berzelius. Before the researches of Berzelius, it had been examined by Guy-Lussac, and Dr John, the latter of whom published an account of its salts.

Taking Dr Prout's view of *merorganization*, as stated by Dr Daubeny, it cannot, we think, be admitted as anything but a fanciful hypothesis in the present state of the science. Whoever has read the detail of the beautiful and interesting researches by which Wöhler and Liebig have established the perfect identity in composition of the cyanic, the insoluble cyanic,

* The vinic acid of Dr Thomson.

and the hydrated cyanic acids, will hesitate long before he admits the necessity of the presence of any *foreign* untraceable body, to produce very different properties in substances giving by analysis the same elements in the same proportions.

We again request our readers to give this book a perusal. It ought to find a place in every English chemist's library.

ART. XXIV.—*Remarks upon Mr J. F. W. Johnston's Critique on the paper upon Specific Heats, published in the July Number of this Journal.* By RICHARD POTTER, Esq., Junior. Communicated by the Author.

IN the last No. of this *Journal*, Mr Johnston favoured the public with a critique on my paper upon the specific heats of some of the metals, printed in the preceding number.

Such critiques are always of some value to the public, as showing in what other points of view the question may be seen, and also, if the critic aspires to scientific notoriety, we often learn from such-like productions something of his peculiar stamp of mind, which enables us to form an estimate of the amount of confidence we may repose in his own experimental labours, and his deductions from them.

Though I have to acknowledge myself obliged to Mr Johnston for the honour he has done me in noticing my paper, and also for his being the cause of my repeating the experiments, earlier, at any rate, than I should have done, on improved methods and with better experience; which have enabled me in the annexed paper to correct some of my former determinations, and also to give a new view on the subject: nevertheless, I must confess that Mr Johnston's critique is liable to several objections.

Omitting many minor points in which he is open to correction, I complain that he has misrepresented me in stating that I adopted "the mean" of my inverse experiments. In such cases it has always appeared to me more correct to study the causes of the discrepancy, and after considering the allowances to be made, to take as true the point most consonant to reason; and not always to strike a blind average. He asserts also, that

I have entered into Mr Dalton's views "regarding heat;" this of course means all Mr Dalton's published views.—If called on for his proof, he would undoubtedly present rather an awkward figure.

His passage on the relative atomic weights of oxygen and hydrogen being irrelevant to the subject, must be taken as a gratuitous favour.

I was in error, however, when I stated Dr Ure's numbers to be taken to oxygen as 7.5, they are to oxygen as 8.—He calls the table, see the third edition of his *Chemical Dictionary*, a table of chemical equivalents, and I had by some unaccountable oversight got the idea that they were ~~to~~ 7.5.

Mr Johnston surely does not think that his opinion will weigh greatly in condemning, as antiquated and obsolete, the atomic weights of three and four years ago, before Dr Thomson's for 1830. And he ought to have seen that the numbers I introduced were Berzelius's older numbers, to show the source whence Dulong and Petit's were derived, and that they were brought to the scale of oxygen 7. for the purpose of a fair comparison.

Mr Johnston appears not to approve of the "tone" I used towards Dulong and Petit, in treating of the way in which they had spoken of others, and in which they had endeavoured to argue away Mr Dalton's prior claims in considering the subject of the laws of specific heats. Before he had said my remarks were unjust, he should have, at any rate, made himself acquainted with the original writings referred to. But Mr Johnston's knowledge on the subject appears to be principally derived from compilations. If he had consulted the original essays of Dulong and Petit, or if he had read Dr Thomson's article on specific heats, carefully, he would have avoided saying, that "all have agreed in considering the method of mixtures as incapable of yielding any precise results."

Dulong and Petit say just the reverse; and some of their most important experiments were made on the old plan of immersion in water. In direct reference to the point in question they say,—"*Parmi les procédés consacrés à la détermination des capacités, ceux dans lesquels on emploie la fusion de la*

glace, ou le mélange des corps avec l'eau, peuvent sans doute, lorsqu'ils sont convenablement dirigés, conduire à des résultats fort exacts ; mais le plus grand nombre des substances sur lesquelles il est indispensable d'opérer peuvent rarement être obtenues en masse suffisante pour que l'une ou l'autre de ces deux méthodes leur soit applicable. Il était donc nécessaire d'avoir recours à un moyen différent. Celui que nous avons adopté nous paraît réunir toutes les conditions désirables." *Annales de Chimie et de Physique* for 1819.

We see that their motive for following another plan was the difficulty of obtaining a sufficient quantity of many of the substances with which they wished to experiment, for the other methods ; and not that which Mr Johnston attributes to them.

In respect to the alloys of gold and silver, which I had used in the shape of coin, I shall have to treat of them in the annexed essay, where it will be seen that I committed an error, rather in considering the higher determinations as correct, instead of the lower ones, than in making right allowance for the alloy ; and I shall only here remark, that Mr Johnston should have calculated the amount of error likely to arise from admixture with a certain proportion of another metal of known specific heat. He would then have found the need of recurring to his only legitimate argument, the probability of error in my numbers from inaccuracy either in the experiments or deductions.

When he says " the old method adopted by Mr Potter is the easiest," he only evinces his own want of experience. If followed in a careless manner it is easy enough. But though it depends chiefly on the reading off of temperatures indicated by a thermometer, the differences in the results of various chemists are a sufficient argument, that it is not without many precautions and some dexterity, to be acquired only by practice, that results in any degree certain and uniform can be obtained. The method pursued by the French philosophers requires certainly a more complex apparatus, but this does not imply a more difficult process in experimenting.

On the subject of opinion in atomic weights I shall say little, as Mr Johnston's opinion, like my own, will have small weight with those best qualified to judge in the matter. But

as to the table where he gives Mr Dalton's numbers, those which he has calculated from Dulong and Petit's, and Dr Thomson's, I cannot avoid making one remark. The relative atomic weights of the metals are generally learnt from their combinations with oxygen and acids; and hence the numbers representing these weights will depend on the number taken for oxygen. Now Mr Dalton's numbers are calculated to oxygen as 7., and Dr Thomson's to oxygen as 8., he has therefore brought Dulong and Petit's numbers, which were originally to oxygen as 1., up to oxygen 8.; and without informing us to which scale he has calculated them, points out the greater differences between the numbers which I had found agreeing best with my experiments on specific heats, namely, those of Mr Dalton, and those made use of by Dulong and Petit, than between those of Dr Thomson and theirs.

ART. XXV.—*On the Specific Heats of certain of the Metals; being a re-examination of the subject.* By R. POTTER, Esq. Junior. Communicated by the Author.

BEING aware that objections had been raised against my former paper on specific heats by Mr Johnston, I had recommenced experimenting on four of the metals before I had seen his critique. I consider it to have happened well that I did so; for if it had been otherwise, I should most probably not have thought it at all necessary to resume the subject.

It was only at the close of my former experiments that I considered it necessary to seek for some more certain method in determining the specific heat of lead; from the differences I had till then experienced in the results given by using equal weights of water and metal. I found that this uncertainty was in a great measure obviated, by employing a larger quantity of the metal, and only a part of its weight of water. With the other metals I had found no reason to suspect that the numbers I had got were not as near the truth as the nature of the experiments would allow. In gold and silver I erred in considering the effect of the metal being in small pieces as likely to produce, with the means I had used, a greater amount of error on the first method than it did in fact. Their being

alloys was considered in fixing the numbers of the second table, as also the counterbalancing effect of the greater hardness which copper communicates to these alloys, similar, though in smaller degree, to what we see in speculum metal, where the specific heat by calculation should be .083, and by experiment, is only about .075.

In all the present experiments, it will be seen that I have used larger quantities of metal than before; and from increased experience I have confidence that I need not now claim that limit of error which was due to the others. And most of the metals of low specific heat, which are by far the most difficult to manage, have been tried on the inverse methods. The results of the whole being as uniform as could be expected by any one conversant with the subject, I consider the numbers I deduce from them as very near approximations. It will be seen from the table that different proportions of water have been used for different metals, and in fact the procedure which would be almost essential for bismuth and lead, would only tend to create error when applied to copper, zinc, or iron, from the different quantities of heat they communicate, which cause proportional differences in the temperatures to be noted.

In the experiments I have continued to use the compound vessel described in my last paper, and remain of the same opinion as to its particular fitness for the purpose. As I use it in general on the first method, its effect on the result is neutralized by having it, previously to putting the water and metal into it, at the temperature which it is expected they will produce together. If Dulong and Petit had attempted that which I call the second method, they would have found their tin-plate vessel ("un vase de fer-blanc très-mince, isolé sur un support à trois pointes,") leading them into great uncertainty and error. I am disposed to consider a great deal of the incorrectness in the results of many chemists to have arisen from their using metallic vessels, and endeavouring to reconcile the results of the inverse methods.

It would have been better to have experimented with pure gold and silver, instead of the alloys in coin; but the same reason which induced Dulong and Petit to recur to their new method has hitherto prevented me from using these metals in

that state. As, however, I now consider the results obtained to be pretty near the truth, I have subjected the specific heats for the alloys to strict calculation, and believe the numbers thus deduced for the pure metals will, on proper proof, be found as correct as the others. It will, however, remain for those who reside in a locality where the pure metals could be procured in sufficient quantity, at a moderate expence, to show how far they are so.

First method, or the metal at 212° immersed in water at common temperatures.

	Weights used.	No. of pieces.	No. of experiments.	Proportion of water.	Specific heats for equal weights.
Iron (best wrought as from smith,)	4140 grs.	1	6	$\frac{1}{2}$	.1115
	4140	1	1	-	.1134
		1	6	-	.1129
		1	6	-	.1131
		1	6	-	.1139
Copper, -	3649	1	6	-	.1188
		1	6	-	.0957
		1	6	-	.0927
Zinc, -	4530	1	6	-	.0948
		1	6	-	.0932
		1	6	-	.0927
		1	7	-	.0920
Silver (coin,)	4327	1	6	-	.0935
		10	6	$\frac{1}{4}$	.0593
		10	6	-	.0589
		10	6	$\frac{1}{2}$	.0605
		10	6	-	.0627
Tin, -	2107	1	6	$\frac{1}{4}$	.0521
	4323	1	6	$\frac{1}{2}$	.0561
		1	1	-	.0555
		1	6	-	.0555
		1	6	-	.0555
Antimony, -	3394	1	2	$\frac{1}{2}$	.0513
		1	6	-	.0512
		1	6	-	.0508
		1	6	-	.0512

	Weights used.	No. of pieces.	No. of experiments.	Proportion of water.	Specific heats for equal weights.
Gold (coin,) -	4918	40	6	$\frac{1}{4}$	.0892
		40	1	-	.0889
		40	1	-	.0399
		40	6	-	.0891
Bismuth, -	3973	1	6	$\frac{1}{4}$	.0320
	3972	1	6	-	.0825
	4234	1	6	$\frac{1}{2}$	.0320
Mercury (purified for barometers,)	4790		2	$\frac{1}{8}$	.0333
			2	-	.0333
			3	-	.0331

Second method, or the metal at common temperatures, immersed in water at 100° to 120°.

Silver (coin,) -	4327 grs.	10	1	$\frac{1}{2}$	.0593
		10	1	-	.0642
Tin, -	3189	many	1	$\frac{1}{4}$	.0548
			1	-	.0557
Gold (coin,) -	4918	40	1	$\frac{1}{8}$	.0389
		40	1	-	.0375
		40	1	-	.0390
		40	1	-	.0375
		40	1	-	.0375
Bismuth, -	4331	18	1	-	.0328
		18	1	$\frac{1}{4}$	.0316
		18	1	$\frac{1}{8}$	.0319
		18	1	-	.0328
		18	1	-	.0339

These results would induce us to adopt for zinc, gold, silver, and bismuth, numbers somewhat lower than my former ones, and for iron one somewhat higher. It will be seen that those for gold and silver do not differ as before, but that the reason I gave for considering the higher numbers then obtained as the most correct, was not well founded, and that, in fact, the lower numbers were as correct as are generally to be obtained.

Though the results of the first method appear to be always

more entitled to credit than those of the second, yet we must allow that the quantities obtained are very liable to be too small, when the experiment is fairly conducted, from the loss of heat sustained in transferring the metal from the hot to the cold water. With every convenience for the purpose, I find, that, even after considerable practice, it requires at least half a second after raising the metal from the boiling water, to shake off from it the hot water, and deposit in the cold. It must in this interval have lost a few degrees of heat, and hence the tendency in the numbers obtained on this method to be too low. The form of the piece of metal will have a considerable effect also; and hence MM. Dulong and Petit having used in their first experiments, (see *Annales de Chimie et de Physique* for 1817,) the form of a flat ring, on account of its presenting a large quantity of surface, their numbers would be particularly liable to be too low. The only object to be gained by using a flat ring is, that it sooner communicates its heat to the cold water. They say, "On a donné à chacun des métaux la forme d'un anneau plat, afin de présenter beaucoup de surface. Ces différents anneaux pesaient d'un à trois kilogrammes."

My own later experiments with most of the metals have been with circular discs of about $2\frac{1}{4}$ inches diameter, and from about $\frac{3}{8}$ th to $\frac{5}{8}$ th inches in thickness, having small appendages for the convenience of experimenting. The common temperature is not so soon acquired as with thinner pieces, but the time taken to arrive at the maximum being noted, the allowance required to be made, which in the above experiments never exceeded half a degree even with iron and copper, is easily determined with every needful accuracy when the *rate of cooling* is but *slow*, as it is in this method of immersing the heated metal in a proper proportion of cold water. My method with the cooling was to throw them together with the boiling water upon a small hair sieve, and then, by reversing the sieve, to transfer them quickly into the cold water.

Taking the above considerations into account, and calculating for pure gold and silver from the known proportions of the alloys with a small allowance for the change of properties caused by the foreign metal, I consider the numbers in the second column of the table below to be very near approxima-

tions ; but such as may *perhaps* be still found in *some* cases wrong to one in the third place of decimals ; and carrying the numbers beyond this I have thought to be claiming a degree of accuracy which the subject is not capable of, on any method yet put in practice.

		Specific heats for equal weights, that of water being unity.	Specific heats according to Dulong and Petit.	
			In 1819.	In 1827.
Iron,	-	.113 (114 ?)	.1100	.1098
Copper,	-	.096	.0949	.0949
Zinc,	-	.094	.0927	.0927
Silver,	-	.059	.0557	.0557
Tin,	-	.056	.0514	
Antimony,	-	.052		.0507
Gold,	-	.034	.0298	
Mercury,	-	.033 +		.0330
Bismuth,	-	.033	.0288	
Lead,	-	.032	.0293	

These numbers are considerably higher in almost every case than those of Dulong and Petit, which have latterly been considered by almost all chemists as very correct determinations. The greatest differences, it will be seen, are in those which they found for the first time on their new method.

But there is one singular point in their table published in 1819, namely, that the numbers found on their new plan for copper, zinc, and silver, are exactly the same to *the fourth place of decimals* as those published in 1817, and found by the plan of immersion. Such a degree of actual coincidence it is absolutely impossible could take place in any separate experiments on the same method, and much less so on any different methods. So that, in regard to Dulong and Petit's latter table, we have only two alternatives ; the one, to consider them as meaning only to say that these substances alone were tried on the new plan which had not been tried on the other ; or that this remarkable coincidence of results was produced by the divers corrections which they mention as necessary to be applied to the actual results of the new plan in the following passage :
 " Il resterait maintenant à indiquer la formule qui sert à cal-

ouler les observations ; mais les détails dans lesquels nous serions forcés d'entrer, sur la manière de faire les diverses corrections qui tiennent à la nature même du procédé, nous entraîneraient dans une discussion que nous réservons de reprendre quand nous publierons les résultats définitifs de toutes les expériences directes que nous avons entreprises à ce sujet."

As to the law respecting the capacities for heat of the ultimate particles of simple bodies, I am now convinced that it stands on a different basis to any which has yet been proposed, and that, in fact, the proposition of Dulong and Petit, that the atoms of all simple bodies have exactly the same capacity for heat, is not capable of proof. In many of the metals, as gold, antimony, bismuth, and perhaps copper, we have good grounds for a suspicion that the law will not hold ; but in silver we have a positive settlement of the point. The single stage only of oxidation that this metal appears to be capable of, and the result of its combination with chlorine, mark, on the only sound principles of the atomic theory, that the weight of its atom is double that which it ought to be if the said law were true. But this anomaly will not be in the least surprising to those who have studied attentively the law of volumes of M. Gay-Lussac. Though this law holds so remarkably for equal or equi-multiple volumes in gases, yet, when we attempt to connect it with atomic considerations, we fall upon the anomalous cases which have divided the opinions of the most celebrated chemists, and produced those different atomic systems which still remain. I believe that the laws of the specific heats of bodies stand in nature on a similar basis to that of the case of volumes in gases ; and, to free the subject from all tendency to hypothesis, as to the total quantity of caloric existing in bodies being proportional to the quantity given out in the experiments for specific heats, the law may be enunciated thus :—that the quantities of heat given out by any masses of simple bodies in descending through the same number of thermometric degrees, is either proportional to the numbers of atoms in the masses, or bears a very simple relation to those numbers.

We shall by this means at any rate avoid being *biased by theoretical ideas* in fixing which ought to be considered binary, and which ternary, or more complex combinations amongst

simple elements; and may then search for other characteristics on which to found our judgment.

The weights of the atoms, on the supposition of their having equal specific heats, which would result from the foregoing experiments, are as follows:—

Formula $d = \frac{m}{c}$	Atomic Weights.	
	for oxygen as 7.	for oxygen as 8.
Iron,	24.7	27.4
Copper,	29.1	32.2
Zinc,	39.7	32.9
Silver,	47.4	52.5
Tin,	50.0	55.3
Antimony,	53.8	59.6
Gold,	82.3	91.1
Mercury,	84.8	93.9
Bismuth,	84.8	93.9
Lead,	87.5	96.8

In the above formula, where we suppose the density of the atoms equal to a constant quantity divided by the specific heat, I have taken for the first column the constant = 28, and for the second = 31. This table will enable the reader to compare these values for d with the numbers in the principal atomic scales, as the last column multiplied by 2 should very nearly agree with Berzelius's scale.

We find a remarkable case in antimony and bismuth; which otherwise present many analogies, that the numbers here found are *almost exactly one-third* more than English chemists generally consider them. Berzelius has latterly taken numbers which *accord* better, but that for bismuth does not accord very well still with *my* experiments.

I confess, that, on the ground of this discrepancy, I expected to have found Dulong and Petit's number for antimony very far wrong. Why they omitted to introduce antimony and mercury into their second table is a point Dulong could best elucidate. It will appear most probable to others that their specific heats did not support the theory.

The problem of the relation between what we call the specific heat of bodies, and the total quantity of heat in them, is

a question for deep consideration ; and one which, in my opinion, has never yet been treated upon in an adequate manner.

We must consider the natural state of bodies, when not undergoing any change, as a state of equilibrium between variable and opposing molecular forces. We see that in measuring the amount of heat given out by a body whilst losing a certain number of degrees of temperature, we only learn the difference in the quantities of heat which was required to produce the equilibrium in the two cases. The full consideration of the problem clearly involves the effect of the whole molecular forces, and belongs to the highest departments of physical science.

In England the consideration of such subjects has too frequently been looked on as speculative and unprofitable, by even those who by their mathematical attainments were qualified for it.

December 6th, 1831.

ART. XXVI.—*Observations on the Organs and mode of Fecundation in Orchideæ and Asclepiadææ.* By ROBERT BROWN, F. R. S., &c. &c.

IN the following pages my principal object is to give an account of some recent observations on the structure and economy of the sexual organs in Orchideæ and Asclepiadææ, the two families of phænogamous plants which have hitherto presented the most important objections to the prevailing theories of vegetable fecundation.

To the account of these observations, which were made chiefly in the course of the present year, will be prefixed a notice, in most cases very slight, of the various opinions that have been held respecting the mode of impregnation in both families: and in concluding the subject of Orchideæ, I shall advert to a few other points of structure in that natural order.

In a separate essay, it is my intention to enter more fully into the details of structure and functions of the sexual organs; and at the same time to give a history, as complete as I am able, of the progress of investigation, with regard both to the

general structure and arrangement of those two families of plants.

ORCHIDEÆ.

The authors whose opinions or conjectures on the mode of impregnation in Orchideæ I have at present to notice, may be divided into such as have considered the direct application of the pollen to the stigma as necessary : and those who,—from certain peculiarities in the structure and relative position of the sexual organs in this family,—have regarded the direct contact of these parts as in many cases difficult or altogether improbable, and have consequently had recourse to other explanations of the function.

In 1760, Haller, the earliest writer of the first class, in describing his *Epipactis*, states that the antheræ or pollen masses, after leaving the cells in which they are originally inclosed, are retained by the process called by him *sustentaculum*, the *rostellum* of Richard, from which they readily fall upon the stigma. He adds, that both in this genus and in *Orchis* the stigma communicates by a fovea or channel with the ovarium.

But as in 1742 he correctly describes the stigma of *Orchis*, and in his account of *Epipactis* notices also the gland derived, as he says, from the *sustentaculum*, and which is introduced between and connects the pollen masses, his opinion on the subject, though not expressed, is distinctly implied even at that period : or as indeed it may be said to have been so early as 1736, when he first described the channel communicating with the ovarium, and considered it as being in the place of a style.

In 1763, Adanson states that the pollen masses are projected on the stigma, of which this description is at least as satisfactory as that of some very recent writers on the subject. He also describes the flower of an Orchideous plant as monandrous, having a bilocular anthera containing pollen which coheres in masses (a view of structure first entertained, but not published, by Bernard de Jussieu), and he correctly marks the relation both of the stamen and placentæ of the ovarium to the divisions of the perianthium.

In 1777, Curtis, in the *Flora Londinensis* in his figure and

account of *Ophrys apifera*, correctly delineates and describes the pollen masses, called by him antheræ, the glands at their base inclosed in distinct coculli or bursiculæ, and the stigma, with the surface of which he represents the masses as coming in contact.

In his second volume, the two lateral adnate lobes of the stigma, and the auriculæ of the column of *Orchis mascula*, are distinctly shown; and these auriculæ, now generally denominated rudimentary stamina, are also delineated in some other species of *Orchis* afterwards figured in the same work.

In 1793, Christian Konrad Sprengel asserts that the pollen masses are applied directly to the secreting or viscid surface on the front of the column, in other words to the stigma, and that insects are generally the agents in this operation.

In 1799, J. K. Wächters supports the same opinion, as far as regards the necessity of direct contact of the pollen masses with the female organ; and this observer was the first who succeeded in artificially impregnating an Orchideous plant, by applying the pollen to the stigma of *Habenaria bifolia*.

In 1799 also, or beginning of 1800, Schkuhr takes the same view of the subject, and observes that the pollen masses, which resist the action of common moisture, are readily dissolved by the viscid fluid of the stigma.

In 1800, Swartz, in adopting the same opinion, notices various ways in which the application of the pollen may be effected in the different tribes of this family, repeats the statement of Schkuhr on the solvent power of the stigma, and in *Blattia Tankervilleæ* describes ducts which convey the absorbed fluid from the stigma to the ovarium.

In 1804, Salisbury asserts that he had succeeded in impregnating many species belonging to different tribes of Orchideæ, by applying the pollen masses to the stigma, whose channel communicating with the cavity of the ovarium, and first noticed by Haller, he also describes.

In 1827, Professor L. C. Treviranus published an account of several experiments made by him in 1824, which satisfactorily prove that impregnation may be effected by the direct application of the pollen to the stigma.

About the end of 1830 a letter from Professor Amici to M.

Mirbel was published, in which that distinguished microscopical observer asserts that in many phænogamous plants the pollen tubes, or *boyaux*, penetrate through the style into the cavity of the ovarium, and are applied directly to the ovula.

In this important communication Orchidæ are not mentioned, but M. Adolphe Brongniart in a note states that he has seen the production of *boyaux* or pollen tubes even in this family ; that here, however, as well as in all the other tribes in which he had examined these tubes, he found them to terminate in the tissue of the stigma.

Of the second class of authors the earliest is Linnæus, who, in 1764, not satisfied either with his own or any other description then given of the stigma, inquires whether the influence of the pollen may not be communicated internally to the ovarium.

In 1770, Schmidel, in an account which he gives of a species of *Epipactis*, describes and figures the upper lip of the stigma, the rostellum of Richard, with its gland both before and after the bursting of the anthera ; and as he denominates that part, before the pollen masses are attached to it, "*stigma virgineum*," he may be considered as belonging to this class.

Koelreuter, the next writer in point of time, and whose essay was published before Linnæus's query appeared, states, in 1775, that the pollen masses, which he denominates naked antheræ, impart their fecundating matter to the surface of the cells of the true antheræ, regarded by him consequently as stigma, and that through this surface it is absorbed and conveyed to the ovarium.

In 1787, Dr Jonathan Stokes conjectures that in Orchidæ, as well as in Asclepiadæ, the male influence, or principle of arrangement, as it is termed by John Hunter, may be conveyed to the embryo without the intervention of air : a repetition certainly of Linnæus's conjecture, with which, however, as it was not published till 1791, he could not have been acquainted.

In 1791, Batsch states that in *Orchis* and *Ophrys*,—and his observation may be extended at least to all *Satyrinæ* or *Ophrydæ*,—the only way in which the mass of pollen can act on the ovarium, is by the retrogradation of the impregnating power through the pedunculus or caudicula of the pollen mass to the

gland beneath it, which he is disposed to refer rather to the stigma than to the anthera.

The late Professor Richard, in 1802, expressly says that fecundation is operated in Orchideæ and Asclepiadæ without a change of place in the stamina; his opinion therefore must be considered identical with that of Batsch, and extended to the whole order.

It might perhaps be inferred from the description which I gave of Orchideæ in a work published in 1810, that my opinion respecting the mode of impregnation agreed with that of Batsch and Richard, though it is not there actually expressed, nor indeed very clearly in another publication of nearly the same date, in which I had occasion to notice this family. But I have since on several occasions more explicitly stated that opinion, which, until lately, I always considered the most probable hypothesis on the subject. At the same time its probability in this family appeared to me somewhat less than in Asclepiadæ. For in Orchideæ a secreting surface in the female organ, apparently destined to act on the pollen without the intervention of any other part, is manifest; and some direct evidence of the fact existed, though not then considered satisfactory. In Asclepiadæ, however, I entertained hardly any doubt on the subject; the only apparently secreting surface of the stigma in that family being occupied by the supposed conductors of the male influence, and no evidence whatever, with which I was acquainted, existing of its action through any other channel.

In 1816 or 1818 I received from the late celebrated Aubert du Petit Thouars some printed sheets of an intended work on Orchideæ, which, with a few alterations, was completed and published in 1822.

From the unfinished work, as well as that which was afterwards published, it appears that this ingenious botanist considered the glutinous substance connecting the grains or lobules of pollen as the "aura seminalis" or fecundating matter; that the elastic pedicel of the pollen mass, existing in part of the family, but according to him not formed before expansion, consists of this gluten; and that in the expanded flower the gluten which has escaped from the pollen is, in all cases, in communication with the stigma.

He describes the Stigma as forming on the surface of the

column a glutinous disk, from which a central thread or cord of the same nature is continued through the style to the cavity of the ovarium, where it divides into three branches, and that each of these is again subdivided into two. The six branches thus formed, are closely applied to the parietes of the ovarium, run down one on each side of the corresponding placenta to its base, each giving off numerous ramuli, which spread themselves among the ovula, and separate them into irregular groups.

Hence, according to this author, a communication is established between the anthera and the ovula, which he adds are impregnated through their surface, and not, as he supposes to be the case in other families, through their funiculus or point of attachment to the placenta.

The remarkable account of the stigma here quoted, though coming from so distinguished and original an observer, and one who had particularly studied this family of plants, seems either to have been entirely overlooked, or in some degree discredited by more recent writers, none of whom, as far as I can find, have even alluded to it. And I confess it entirely escaped me until after I had made the observations which will be stated in the present essay, and which confirm its accuracy as to the existence and course of the parietal cords, though not as to their nature and origin.

In 1824 Professor Link expresses his opinion that the rostellum of Richard is without doubt the true stigma.

In 1829 Mr Lindley, who for several years has particularly studied, and has lately published part of a valuable systematic work on Orchideous plants, states that in this family impregnation takes effect by absorption from the pollen masses through their gland into the stigmatic channel.

In 1830, in his *Introduction to the Natural System of Botany*, the same statement is repeated; and in this work it also appears that he regards the glands to which the pollen masses become attached in Ophrydææ as derived from the stamen, and not belonging to the stigma, as in 1810 I had described them. It would even appear, from a passage in his systematic work published in the same year, that he considers the analogous glands, existing in most other tribes of Orchideæ, as equally belonging to the stamen: in his "introduction," however, he refers them to the stigma in all cases except in Ophrydææ.

Towards the end of 1830 the first part of Mr Francis Bauer's *Illustrations of Orchideous Plants*, edited by Mr Lindley, was published.

From this work, of the importance and beauty of which it is impossible to speak too highly, it may be collected that Mr Bauer's opinion or theory of impregnation in Orchideæ does not materially differ from that of Batsch, Richard, and other more recent writers. From one of the figures it appears that this theory had occurred to him as early as 1792; and in another figure, bearing the same date, he has accurately represented the structure of the grains of pollen in a plant belonging to Ophrydeæ, a structure which I had not ascertained in that tribe till 1806. Although Mr Bauer's theory is essentially the same as that of Batsch and Richard, yet there are some points in which it may be considered peculiar; and chiefly in his supposing impregnation to take effect long before the expansion of the flower, at a time when the sexual organs are so placed with relation to each other that the fecundating matter, believed by him to pass from the pollen mass through its caudicula, where that part exists, to the gland attached to it, may be readily communicated to the stigma, with which the gland is then either in absolute contact or closely approximated. The more important points of this account may be extended to nearly the whole order, but it is strictly applicable only to Satyrinæ or Ophrydeæ, a tribe in which Mr Bauer seems, with Mr Lindley, to consider the glands as belonging to the stamen and not to the stigma. In those genera of this tribe in which the glands are included in a pouch or bursicula, he describes and figures perforations in the back of the pouch, through which the fecundating matter is communicated from the glands to the stigma; and one of the figures is intended to represent a gland in the act of parting with the fecundating matter.

It is impossible to judge correctly of Mr Bauer's theory until all the proofs and arguments in its favour are adduced. I may observe, however, that those already published are by no means satisfactory to me.

For, in the first place, in the very early stage in which, according to this theory, impregnation is supposed to be effected, it appears to me that the pollen is not in a state to im-

part its fecundating matter, nor the stigma to receive it ; and it may be added, though this is of less weight, that the ovula have neither acquired the usual degree of developement, nor that position which they afterwards take, and which gives the apex of the nucleus or point of impregnation the proper direction, with regard to the supposed impregnating surface.

Secondly, in the figure which may be said to exhibit a demonstration of the correctness of the theory,—in that, namely, representing the gland in the act of parting with the fecundating matter,—the magnifying power employed (which is only fifteen times) is surely insufficient for the establishment of a fact of this kind ; while the disengagement of minute granules, which no doubt often takes place when the gland is immersed in water, may readily be accounted for in another way.

Thirdly, I have never been able to find those perforations, represented by Mr Bauer, in the bursiculæ of *Orchis* and *Ophrys*, and the existence of which in these genera is essential to his hypothesis.

And, lastly, the appearance of the stigma in *Bletia Tankervilleæ* after impregnation, as he believes, according to my view of the subject would rather prove that it was in a state capable of acting upon, but had not yet received the fecundating matter from, the anthera.

In thus venturing to differ from so accurate and experienced an observer as Mr Bauer, on a subject which he has for many years minutely studied and so beautifully illustrated, I am well aware how great a risk I incur of being myself found in error.

I am very desirous, however, that the perusal of this sketch of the various statements that have appeared on the question of impregnation, with the greater part of which he is at present probably unacquainted, should induce him to reexamine the facts and arguments by which his own opinion on this subject is supported. He will thus either succeed in establishing his theory on more satisfactory grounds, or, if the examination should prove unfavourable, he will, I am persuaded, from his well known candour, as readily abandon it.

The notice now given of the opinions of botanists on impregnation in Orchideæ brings the subject down to the spring

of the present year, when from circumstances, which I may hereafter have occasion to advert to, my attention was directed to this family of plants, the particular study of which I had for a long time discontinued.

In reviewing notes respecting them, made many years ago, I found some points merely hinted at, or imperfectly made out, which seemed deserving of further examination; and in the course of this inquiry, other observations of at least equal importance suggested themselves.

I now proceed to state, in some cases briefly, in others at greater length, the results of this investigation.

The first question that occupied me was, the relation which the lateral and generally rudimentary Stamina bear to the other parts of the flower.

Into this subject I had in part entered in my *Observations on Apostasia*, published by Dr Wallich in his splendid "*Plantæ Asiaticæ Rariores*," and had then considered it probable that in all cases these stamina, in whatever state of development they were found, belonged to a different series from the middle and usually fertile stamen; in other words, were placed opposite to the two lateral divisions of the inner series of the perianthium. In 1810, however, when I first advanced my hypothesis (See Note A) of the true nature of these processes of the column, I supposed, though the opinion was not then expressed, that they formed the complement of the outer series of stamina; a view which has been since very generally adopted, especially by Dr Von Martius, who has given it in a stenographic formula, and by Mr Lindley, who has exhibited the relative position of parts in this family in a diagram. A careful examination of the structure of the column in various tribes of the order, chiefly by means of transverse sections, has fully confirmed the opinion I entertained when treating of *Apostasia*; and more particularly established the fact in *Cypripedium*, in which these lateral stamina are perfectly developed.

The next point examined was the composition of the Stigma with the relation of its lobes or divisions to the other parts of the flower, and especially to the supposed component parts of the ovarium. On this subject very little information is to be obtained from the writings of botanists, most of whom have

contented themselves with describing the stigma as a disk, a fovea glutinosa, a secreting surface, or viscid space in front of the column. The late celebrated Richard, however, who adverts to the occasional existence of two lateral processes of his gynizus, may be supposed to have had more correct notions of its composition: and it may also be observed, that in Curtis's plate already referred to, and still more distinctly in Mr Bauer's figure of *Orchis mascula*, the two lateral lobes are represented as distinct, corresponding very exactly with Haller's description, in 1742, of the stigma in this genus.

(To be continued.)

ART. XXVII.—*Summary of Meteorological Observations made at Kendal in December 1830, and January, February, March, April, May, June, July, and August 1831.* By Mr SAMUEL MARSHALL. Communicated by the Author.

State of the Barometer, Thermometer, &c. in Kendal, in December 1830.

	Barometer.	Inches.
Maximum on the 16th,	- -	30.23
Minimum on the 31st,	- -	28.83
Mean height,	- -	29.47

	Thermometer.	
Maximum on the 7th,	- -	45°
Minimum on the 26th and 27th,	- -	16°
Mean height,	- -	34.10°

Quantity of rain, 2.082 inches.

Number of rainy days, 9.

Prevalent winds, east and west.

The barometer and thermometer have both been very variable during the month; and what is remarkable, the barometer continued the highest during the middle of the month, when the weather was most unsettled and rainy. The frost was very severe, and even intense the last ten days of the year, except that in the last two days a thaw succeeded, and more than one-half the quantity of rain for the month was measured

on the morning of the 31st. We have had several most splendid appearances of the Aurora Borealis. The brightness and variety have been remarkable; the bright light in the north, streamers and luminous arches. On the evening of the 25th, the streamers were more numerous, and exhibited more variety of colour, than have been observed in any appearance of this kind for many years past; almost all the prismatic colours were observed in them. Several bows of light appeared on different parts of the evening, and sometimes two or three were seen at a time. They were mostly about 30° high, and always in the north part of the heavens.

January 1831.

Barometer.			Inches.
Maximum on the 7th,	-	-	30.40
Minimum on the 21st,	-	-	29.08
Mean height,	-	-	29.71

Thermometer.			
Maximum on the 15th,	-	-	48°
Minimum on the 30th,	-	-	18°
Mean height,	-	-	33.37°

Quantity of rain, 1.619 inches.

Number of rainy day, 6.

Prevalent wind, north west.

We have been frequently visited this month by the Aurora Borealis, though mostly unattended by streamers, consisting of a bright white light in the north, of considerable intensity; but on the evening of the 11th, the streamers were more numerous, of longer continuance, and more vivid than any that have been observed either in the winter or autumn. The barometer does not seem to be affected by the appearance of these singular and interesting visitors, though considerable notice has been taken to ascertain the fact. During the last and foregoing months, the appearance of the Aurora Borealis generally preceded a change in the weather. This has not been the case during the present. On the 26th we had a considerable fall of snow, which, however, did not remain long on the ground, and in the last day of the month a still thicker

fall, the wind having been in the N. and N. W. chiefly for several preceding days. The weather has been very dry and generally frosty, and from the 4th to the 18th, no rain at all. The winds from the N. W., N., and N. E. have prevailed for nineteen days, and the weather at these times was very cold and piercing.

February.

Barometer.			Inches.
Maximum on the 23d,	-	-	30.20
Minimum on the 26th,	-	-	28.87
Mean height,	-	-	29.59

Thermometer.

Maximum on the 9th and 11th,	-	-	50.5°
Minimum on the 4th,	-	-	19°
Mean height,	-	-	38.14°

Quantity of rain, 8.208 inches.

Number of rainy days, 17.

Prevalent wind, south west.

The frost, which was so intense the latter part of last month and the beginning of this, was succeeded by a thaw so rapid that the snow and ice melted very quickly, and occasioned a flood greater than is remembered by any person now living. The houses in the lower parts of the town were from four to five feet high in water, the river overflowed its banks from the morning of the 8th to the morning of the 10th. The coaches from the north could not pass through the snow for several days at the beginning of the month. The barometer and thermometer have varied considerably. We have had very little frost since the 7th, and the wind has been chiefly in the S. W. The quantity of rain taken in the morning of the 9th for the preceding twenty-four hours was 2.441 inches, the greatest quantity which has fallen in the space of twenty-four hours for the last nine years, excepting once, which was on the 14th October 1829, and which amounted to 2.533 inches. The Aurora Borealis, which has for several months been so prevalent, has not been observed this month, owing, perhaps, to the cloudy state of the atmosphere.

March 1831.

Barometer.			Inches.
Maximum on the 31st,	-	-	30.49
Minimum on the 6th,	-	-	28.88
Mean height,	-	-	29.66

Thermometer.			
Maximum on the 20th,	-	-	55°
Minimum on the 24th,	-	-	31°
Mean height,	-	-	43.20°

Quantity of rain, 6.208 inches.

Number of rainy days, 17.

Prevalent wind, west.

During this month we have had no frost of any consequence, as the thermometer has been but once at the freezing-point, and once below that point. In the latter part of the month, from the 18th, the weather has been dry and cold, and the wind chiefly from the N. E. and E. From the beginning of the month to the 18th, we had extremely wet weather, and 5.905 inches of rain fell in that period. The barometer has kept high, when the long continuance of rain is taken into account. About the middle of the month the Aurora Borealis was repeatedly seen, and sometimes very brilliant, with streamers. We have already had during the present year, 15.855 inches of rain, and forty wet days: and we may conclude from this that the season has been a wet one; but the greatest part of the rain has fallen within the last two months. The equinoctial gales began about the 12th, and the winds were very strong and gusty to near the end of the month.

April.

Barometer.			Inches.
Maximum on the 1st,	-	-	30.46
Minimum on the 30th,	-	-	28.57
Mean height,	-	-	29.54

Thermometer.			
Maximum on the 16th,	-	-	61°
Minimum on the 4th,	-	-	26.5°
Mean height,	-	-	47.82°

Quantity of rain, 2.433 inches.

Number of rainy days, 11.

Prevalent winds, west.

The barometer has been remarkably variable this month. The thermometer has not been at the freezing point nor below it since the 5th. Excepting in one instance, on the 10th, we have not had much rain, at least not more than gentle showers occasionally, sufficient to keep the ground moist, and which have promoted vegetation to an astonishing degree. The richness of the season has seldom been surpassed, and rarely equalled. The Aurora Borealis has been seen a few times during the month. Total quantity of rain for this year, 18.288 inches.

May.

Barometer.	Inches.
Maximum on the 11th, - -	30.10
Minimum on the 1st, - -	28.75
Mean height, - -	29.66

Thermometer.

Maximum on the 23d, 25th, and 26th, -	71°
Minimum on the 7th, - -	28.5°
Mean height, - -	54.11°

Quantity of rain, 0.721 inch.

Number of rainy days, 8.

Prevalent wind, north east.

The most distinguishing character of the weather in this month has been its extreme dryness. The winds from the N. E., E. and S. E., have prevailed twenty days during the course of this month. There has seldom been a single day without a part of it having a wind from some quarter about the east. The early part of the month was very cold, and the frost which then occurred, not only arrested vegetation very materially, but killed most of the tender plants, and injured the blossoms, which were most abundant. The frost being followed by the prevalence of dry easterly winds, has been most unfavourable to the products of the soil. In the early parts of the month, the Aurora Borealis was seen a few times. Snow covered the hills pretty thick on the 6th, and we had several snow showers on that day. The dryness of the month is evident from the quantity of rain being less than three-fourths

of an inch, and the greater part of this fell on the first three days of the month.

June.

	Barometer.	Inches.
Maximum on the 23d,	- -	29.98
Minimum on the 11th,	- -	29.16
Mean height,	- -	29.69

Thermometer.

Maximum on the 4th,	- -	70°
Minimum on the 7th,	- -	40°
Mean height,	- -	58.06°

Quantity of rain, 2.682 inches.

Number of rainy days, 16.

Prevalent wind, west.

About the beginning of the month there was a good deal of thunder, but mostly at a distance. The weather has generally been showery, and not very favourable for the hay harvest.

The number of days on which rain has fallen is greater than is usual in this month. The evenings have been frequently attended with a remarkably red sky, but this appearance has not often been succeeded by fine days, which is mostly the case. The prevalent wind in the day time was E. or N.E. to the 8th, which is about three weeks after the usual time when the dry easterly winds of the spring subside. The weather after that time was hot, and the rainy days were mostly sultry.

July.

	Barometer.	Inches.
Maximum on the 6th,	- -	30.16
Minimum on the 21st,	- -	29.30
Mean height,	- -	29.76

Thermometer.

Maximum on the 31st,	- -	73°
Minimum on the 24th,	- -	44°
Mean height,	- -	60.50°

Quantity of rain, 4.081 inches.

Number of rainy days, 17.

Prevalent wind, west.

The weather during this month has been very hot and sultry, and mostly favourable to the hay harvest, though there have been more rainy days than dry ones. Not much thunder, but what was at a distance, unless we except a heavy thunder storm on the 31st. The sky has been mostly cloudy, which has moderated a season that would otherwise have been extremely hot.

Though in many other parts of the country the thermometer appears to have been frequently from 80° to 90° in the shade, yet in this town it never rose above 73°. In the nights it ranged mostly from 50° to 60°.

August.

Barometer.

Maximum on the 22d,	-	-	30.10
Minimum on the 25th,	-	-	29.38
Mean height,	-	-	29.73

Thermometer.

Maximum on the 3d and 6th,	-	-	73°
Minimum on the 18th.	-	-	44°
Mean height,	-	-	60.84°

Quantity of rain, 3.899 inches.

Number of rainy days, 14.

Prevalent wind, west.

The weather about the middle, and in the preceding part of the month was hot and sultry, though attended with frequent showers, the barometer continuing mostly high. The thermometer indicated a considerable degree of heat, but it has not during the summer been higher than 73° in the shade.

Though this has been a much hotter summer than the last, yet the thermometer last year stood at 81° on the 31st July.

The mean temperature of this month exceeds that of the corresponding one in last year nearly 6°, and the average of the hottest month in last year (July) was 58. 59°. The total quantity of rain for the present year to the end of this month is 29.671 inches, being nearly 3½ inches less than for the same period in last year.

ART. XXVIII.—REGISTER OF THE BAROMETER, THERMOMETER, AND RAIN-GAGE, kept at *Edinburgh*. By ALMA ADIE, Esq. F. R. S. Edin.

THE Observations contained in the following Register were made behind the Regent Terrace, on the south-east slope of the Calton Hill, by means of very nice instruments, constructed by Mr Adie. The height of the instruments is 246 feet above the medium level of the sea. The morning and evening observations were made about 10 A.M. and 10 P.M.

SEPTEMBER 1831.

OCTOBER 1831.

NOVEMBER 1831.

D. of Week.	Day of Month.	Thermometer.			Register Therm.			Barometer.		Rain.	Thermometer.			Register Therm.			Barometer.		Rain.
		Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.		Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.	
T. 1.	1	58	52	55	48	60	54	29.70	28.77	.40	58	52	55	48	60	54	29.35	28.35	.03
T. 2.	2	53	54	54.5	46	58	52	29.20	28.61	.27	43	49	46	40	53	46.5	29.05	28.05	.21
T. 3.	3	55	54	54.5	44	63	53.5	29.18	28.59		34	43	38.5	34	43	38.5	29.08	28.51	
T. 4.	4	53	54	53.5	44	59	51.5	29.12	28.53	.27	38	38	38	38	38	38	29.52	29.51	
T. 5.	5	56	52	54	47	61	54	29.12	28.53	.21	38	38	38	38	38	38	29.17	28.78	.43
T. 6.	6	59	55	56.5	47	63	56.5	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.19
T. 7.	7	58	54	56	47	61	54.5	29.12	28.53		38	38	38	38	38	38	29.17	28.78	
T. 8.	8	56	54	55	47	57	54.5	29.12	28.53	.21	38	38	38	38	38	38	29.17	28.78	
T. 9.	9	56	54	55	47	57	54.5	29.12	28.53	.21	38	38	38	38	38	38	29.17	28.78	
T. 10.	10	56	54	55	47	57	54.5	29.12	28.53	.21	38	38	38	38	38	38	29.17	28.78	
T. 11.	11	57	56	56.5	49	59	54	29.12	28.53	.15	38	38	38	38	38	38	29.17	28.78	.07
T. 12.	12	56	55	55.5	49	59	54	29.12	28.53	.15	38	38	38	38	38	38	29.17	28.78	
T. 13.	13	56	55	55.5	49	59	54	29.12	28.53	.15	38	38	38	38	38	38	29.17	28.78	
T. 14.	14	57	56	56.5	49	61	56	29.12	28.53	.19	38	38	38	38	38	38	29.17	28.78	
T. 15.	15	56	54	55.5	49	61	56	29.12	28.53	.02	38	38	38	38	38	38	29.17	28.78	.06
T. 16.	16	57	54	56.5	50	61	57	29.12	28.53		38	38	38	38	38	38	29.17	28.78	
T. 17.	17	56	54	55	49	60	55	29.12	28.53	.06	38	38	38	38	38	38	29.17	28.78	
T. 18.	18	56	54	55	49	60	55	29.12	28.53		38	38	38	38	38	38	29.17	28.78	
T. 19.	19	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 20.	20	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 21.	21	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 22.	22	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 23.	23	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 24.	24	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 25.	25	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 26.	26	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 27.	27	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 28.	28	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 29.	29	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
T. 30.	30	56	54	55	49	60	55	29.12	28.53	.11	38	38	38	38	38	38	29.17	28.78	.08
Sum.		1795	1601	1698.5	1482	1835	1638.5	291.55	281.46	1.55	1799	1196	1292.5	1096	1557	1211.5	286.38	286.64	2.95
Mean.		57.5	53.47	55.48	49.4	61.17	55.98	29.13	28.715		41.65	39.87	40.75	35.58	45.23	40.38	28.446	28.555	

THE
EDINBURGH
JOURNAL OF SCIENCE.

ART. I.—*Memoir of the Life of Thomas Young, M. D. F. R. S.*
Foreign Associate of the Royal Institute of France, &c. &c.

DR THOMAS YOUNG was born at Milverton in Somersetshire, the 13th of June 1773. He was the eldest of ten children of Mr Thomas Young of that place, and his mother was a niece of Dr Brocklesby, a physician of eminence in the metropolis. His parents were both of them of the society of Quakers.

From a very early period Dr Young was chiefly an inmate in the family of his maternal grandfather, Mr Robert Davies, of Minehead; a gentleman who amidst mercantile avocations, though no very deep or accurate scholar, had cultivated a taste for classical literature, which it was his earnest endeavour to impress upon the mind of his grandson. It is stated that whilst domesticated with him, he had learnt to read with fluency when he was two years old, and that soon after this, in the intervals of his attendance on a village school-mistress, he was made to commit to memory a number of English poems, and even some Latin ones, the words of which he retained without difficulty, although at the time unacquainted with their meaning.

Before he was six years old, he attended the seminary of a dissenting minister, and went afterwards to a school at Bristol, where he remained about a year and a-half.

His father had a neighbour, a man of great ingenuity, by

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profession a land-surveyor and land-steward ; and in his office, during his holidays, he was indulged with the use of mathematical and philosophical instruments, together with the perusal of three volumes of a dictionary of arts and sciences, which he also found there. These were to him sources of instruction and delight of which he seemed never to weary, and which, thus accidentally thrown in his way, had probably no small influence on the issues of his future life.

In 1782, he was sent to the school of a Mr Thompson, at Compton, in Dorsetshire, of whom he was always accustomed to speak with great respect, as a person of an enlarged and liberal mind ; and who, possessed of a moderate and miscellaneous library, permitted and encouraged his scholars to turn it to their profit.

Here Young went through the ordinary course of Greek and Latin classics, together with the elementary parts of the mathematics ; and by rising earlier and sitting up later than his companions, with the assistance of a school-fellow who had some French and Italian books, he rendered himself tolerably familiar with those languages. He had acquired, in his visits to his father's neighbour, the art of land surveying, and the amusement of his walks was to measure heights with a quadrant.

The next study he undertook was botany, and for the sake of examining the plants which he gathered, he attempted the construction of a microscope from the description of Benjamin Martin. This led him to optics ; but in order to make his microscope, he found it necessary to procure a lathe. Every thing then gave way to a passion for turning, and science was forgotten for the acquirement of manual dexterity ; until falling upon a demonstration in Martin which exhibited some fluxional symbols, he was never satisfied till he had read and mastered a short introduction to the doctrine of fluxions.

Mr Thompson had left in his way a Hebrew Bible. He began by enabling himself to read a few chapters, and was soon absorbed in the study of the principal Oriental languages. At the age of fourteen, when he quitted Mr Thompson's school, he was thus more or less versed in Greek, Latin, French, Italian, Hebrew, Persic, and Arabic ; and in forming the characters of those languages, he had already acquired much of the

beauty and accuracy of penmanship which was afterwards so remarkable in his copies of Greek compositions, as well as of those subjects connected with the literature of ancient Egypt.

In 1787, the friends of Dr Young were beginning to think seriously of the line in life which might be most advantageously taken for a youth of such extraordinary promise. When at the house of one of his relations he accidentally met a connexion of Mr David Barclay, of Youngsbury, in Hertfordshire, who was then wishing to form an arrangement for the education of his grandson, and through the intervention of Sir William Watson, it was agreed that Dr Young and the grandson of Mr Barclay should pursue their studies together, under a private tutor, in Mr Barclay's house. The tutor who was engaged found a situation of greater permanence, and never came; so that two boys being left together, whose ages differed only about a year and a-half, Young, then little more than fourteen, took upon himself provisionally, the office of preceptor.

When about the age of fourteen, he was attacked by symptoms of what was feared to be incipient consumption. But under the attendance of his uncle Dr Brocklesby, and Baron Dimsdale, he recovered his health, without suffering any ultimate inconvenience, and was enabled for the most part, to pursue his labours through the whole duration of his indisposition, merely relieving his attention by what, to him, stood in the place of repose—a course of Greek reading in such authors as amused the weariness of his confinement.

In the five years between 1787 and 1792, residing during the summers in Hertfordshire, and for some months of the winter in London, with no other assistance than that of a few occasional masters in the latter place, he had rendered himself singularly familiar with the great poets and philosophers of antiquity, keeping ample notes of his daily studies.

He had acquired a great facility in writing Latin. He composed Greek verses which stood the test of the criticism of the first scholars of the day, and read a good deal of the higher mathematics. His amusements were the studies of botany and zoology, and to entomology in particular he at that time gave great attention.

In the winters of 1790 and 1791, having prepared himself by previous reading, he attended the lectures of Dr Higgins in chemistry, and began to make some simple experiments of his own on a small scale. But he was afterwards accustomed to say, that at no period of his life was he particularly fond of repeating experiments, or even of very frequently attempting to originate new ones.

His uncle Dr Brocklesby had at this time desired to receive from him a regular report of his literary and scientific pursuits, intending to take upon himself the supervision of his further education for the practice of physic, which was the line he recommended him to adopt; and having communicated some of his Greek translations to Mr Burke and Mr Windham, with both of whom Dr Brocklesby had lived in intimacy, an acquaintance with these two distinguished persons ensued; in the course of which Mr Burke was so greatly struck with the reach of his talents and the extent of his acquirements, more particularly by his great and accurate knowledge of the Greek language, that Dr Young may be considered as in no small degree indebted to the good offices of that eminent statesman, for the extent of interest which his uncle took from this period in his future settlement in life.

It was in 1791 that he made his first communications to the press, through the *Monthly Review* and the *Gentleman's Magazine*, being Greek criticism, chemical theories, and remarks on botany and entomology.

Towards the end of 1792, Dr Young established himself in lodgings in Westminster, in which he resided about two years, for the purpose of pursuing his medical studies, attending the lectures of Baillie and Cruickshank in the Hunterian school of anatomy; and he was during that period amongst the most diligent of the pupils of St Bartholomew's Hospital.

In 1793, he made a tour in the west of England, principally with a view of studying the mineralogy of Cornwall; and about this time having been introduced to the acquaintance of Charles Duke of Richmond, who had long been a friend of his uncle's, and was then Master-General of the Ordnance, he was offered by his Grace the situation of assistant-secretary in his house. He felt that this was an opportunity for entering into

public life which might lead to advantage and distinction. Mr Burke and Mr Windham recommended him rather to proceed to Cambridge and study the law ; but after some consideration of these conflicting proposals, he determined to adhere to the pursuits of science, and to proceed to the practice of physic, as most congenial both to his predilection and his habits, and to which the position occupied by his uncle appeared to offer a natural introduction.

In this year he gave to the Royal Society his Observations on Vision, and his Theory of the Muscularity of the Crystalline Lens of the Eye, which became the subject of much discussion; and John Hunter immediately laid claim to having previously made the discovery. Dr Young was soon afterwards elected a Fellow of the Royal Society, when he had just completed his twenty-first year.

In the autumn of 1794 he went to Edinburgh, and there attended the lectures of Drs Black, Monro, and Gregory. He pursued every branch of study in that university with his accustomed intensity, but made the physical sciences more peculiarly the objects of his research. He now separated himself from the Society of Quakers, and amidst his medical, scientific, and classical labours, he determined on cultivating some of those arts in which he considered that his early education had left him deficient.

Towards the close of 1795 he went to the university of Gottingen, where he took his doctor's degree. His extraordinary attainments, and the almost incredible industry with which he pursued his studies in all their variety, excited the wonder of the laborious school in which he had now placed himself. He found their academical library peculiarly rich in works of reference ; and in composing his inaugural dissertation, "*De Corporis Humani Viribus Conservatricibus*," he left few volumes unconsulted which had any connexion with the subject on which he was treating.

In all periods of his life Dr Young was entirely exempt from those dissipations into which young persons unhappily very generally fall, and here, as at Edinburgh, he diversified his graver studies by cultivating skill in bodily exercises. He took lessons in horsemanship, in which he always had great pleasure,

and practised under various masters all sorts of feats of personal agility, in which he excelled to an extraordinary degree.

The victories of the French at this time prevented him from visiting Italy, which he had intended to do previously to his return to England ; and unwilling to be deficient in any species of knowledge, he proceeded to Dresden ; where he spent some time for the purpose of studying the works of Italian art in the galleries of that city, and to compare what he saw with that which he had learnt of them from the lectures of the professors of Gottingen. Before returning home, he completed his stay on the continent by a short visit to Berlin.

Dr Young, during his residence in Germany, had gained a very general and accurate acquaintance with the language and literature of that country, which he kept up throughout his life ; but he remarked that he found in Germany a love of new inventions, singularly, and somewhat pedantically, combined with the habit of systematizing older ones, and giving an importance to things in themselves trifling, which in his case rather confirmed an original habit of dwelling on minutiae more than his subsequent experience led him to think was advantageous.

In consequence of some new regulations of the College of Physicians, which had taken place during his residence abroad, he found himself precluded from immediately practising as a licentiate in London ; he therefore entered himself as a fellow-commoner in Emanuel College, Cambridge.

Dr Brocklesby died in December 1797, when the larger part of his fortune was inherited by his nephew, Mr Beeby ; the remainder, with his house, his books, and his pictures, was left to Dr Young. He now found himself in circumstances of independence, surrounded by a circle of academical friends and associates, and formed many friendships in distinguished and highly cultivated society, which he continued to prize and to enjoy through life.

When his necessary residence at college was completed, Dr Young settled himself as a physician in London, in Welbeck Street, where he continued to reside during twenty-five years. It was not long, however, before he accepted the situation of Professor of Natural Philosophy in the Royal Institution,

where he was for two years colleague as lecturer with Sir Humphry Davy. The first volume of the Journals of the Royal Institution and a part of the second were edited, and for the most part composed by him. He gave two Bakerian lectures on the subjects of light and colours to the Royal Society, and in 1802 he published a Syllabus of a course of lectures on natural and experimental philosophy, with mathematical demonstrations of the most important theorems in mechanics and optics; and containing the first publication of his discovery of the general law of the interference of light, being the application of a principle which has since been universally appreciated as one of the greatest discoveries since the time of Newton, and which has subsequently changed the whole face of optical science.

In the summer of 1802 Dr Young accompanied the present Duke of Richmond and his brother, Lord George Lennox, in his medical capacity, to Rouen in Normandy, with their tutor, Mr Vincent; and in an excursion from thence to Paris, was first present at the discussions of the National Institute of France, at that time attended by Napoleon; where he made the acquaintance of several leading members of that distinguished body, into which he himself was eventually elected. On his return he was constituted foreign secretary to the Royal Society, an office which he held during life, being long their senior officer, and always one of the leading and most efficient members of their council.

In 1804, Dr Young married Miss Eliza Maxwell, daughter of James Primrose Maxwell, Esq. of Cavendish Square, who has lived to lament his loss, after an union which was attended with uninterrupted happiness.

At this time he resigned his office as lecturer to the Royal Institution, it being thought by his friends that his holding it longer would be likely to interfere with his success as a medical practitioner.

In 1807, Dr Young published his "Course of Lectures on Natural Philosophy and the Mechanical Arts," in two volumes quarto. This elaborate work he stated to have been the result of the unremitting application of five years.

The booksellers engaged in this publication failed at the

moment of its coming out, which greatly injured the immediate sale of the work.

For sixteen successive years from the period of his marriage, Dr Young passed his winters in London and his summers at Worthing, having been in 1810 appointed physician to St George's Hospital. In his profession his published labours would prove him to have been one of the most learned and scientific physicians, and his judgment and acuteness were equally great: but in the practice of medicine Dr Young was not one of those who were likely to win the most extended occupation amongst the multitude. He was averse to some of the ordinary methods by which it is acquired. He never affected an assurance which he did not feel, and had perhaps rather a tendency to fear the injurious effects which might eventually result from the application of powerful remedies, than to an overweening confidence in their immediate efficacy. His treatises bear the same impress. That on Consumption, is a most striking instance of his assiduity in collecting all recorded facts, and his abstinence in drawing inferences from isolated cases, or putting forth that which he did not feel was established with certainty.

Dr Young afterwards published a "*Syllabus of Lectures on the Elements of the Medical Sciences*," as delivered by him at the Middlesex Hospital, and his "*Introduction to Medical Literature, including a System of Practical Nosology*,"—the latter a work of great labour, forming like his *Natural Philosophy*, a text book of the highest practical utility, and accompanied by a mass of references.

To the larger of these works he prefixed a "Preliminary Essay on the Study of Physic," in which he gives a singular picture of what, in his opinion, is required to constitute a well-educated physician; enumerating nearly every possible quality of which man could wish, but of which few could hope, the attainment.

Dr Young contributed to the *Quarterly Review* a variety of articles, literary and scientific. He first engaged, at the suggestion of Mr George Ellis, one of his most intimate and most valued friends, to furnish those on medical subjects to that work. But his communications soon branched into other lines, many of them connected with the higher departments of science,

and containing the results of some of his most laboured researches. The Review of *Adelung's Mithridates*, vol. x. October 1813, is perhaps the most remarkable, not only from the immense knowledge it displays of the structure of almost all languages, but as having been the composition which first led him to the investigation of the lost literature of ancient Egypt.

In the year 1814, Sir William Rouse Boughton had brought with him from Egypt some fragments of Papyri, which he put into the hands of Dr Young; the fragment of the Rosetta stone having been about this time deposited in the British Museum; and a correct copy of its three inscriptions having been engraved and circulated by the Society of Antiquaries. Dr Young first proceeded to examine the enchorial inscription, and afterwards the sacred characters, and after a minute comparison of these documents, he was enabled to attach some "Remarks on Egyptian Papyri, and on the inscription of Rosetta," containing an interpretation of the principal parts of both the Egyptian inscriptions on the pillar, to a paper of Sir William Boughton's, which was published by the Society of Antiquaries in 1815, in the eighteenth volume of the *Archæologia*.

Dr Young now found he had discovered a key to the lost literature of ancient Egypt. He had occupied himself, though without deriving from it the assistance he had at first expected, in the study of the Coptic and Thebaic version of the Scriptures; but having satisfied himself of the nature and origin of the enchorial character, he produced the result to the world anonymously in the *Museum Criticum of Cambridge*, part the sixth, published in 1815.

In 1816, he printed and circulated two additional letters relating to his hieroglyphical discoveries, and the inscription of Rosetta; the first addressed to the Archduke John of Austria, who had recently been in this country, the other to M. Akerblad. These letters announce the progress of the discovery of the relation between the Egyptian characters and hieroglyphics, forming the basis on which Dr Young continued his inquiries, as well as of the system afterwards carried further in its details by M. Champollion, whose attention had long been directed to similar studies, and in which he has since so great-

ly distinguished himself. The letters were first published when reprinted in the seventh number of the *Museum Criticum* in 1821 ; and were, with the former letters in that work, the earliest announcement of the discovery of a key to a character which had remained uninterpreted for ages.

In the same year he agreed to furnish various articles to the *Supplement to the Encyclopædia Britannica*, and under the head "EGYPT," he first brought out the whole results of his discoveries in a perfect and concentrated form.

To this work Dr Young furnished sixty-three articles, scientific, biographical, and literary ; the signature by which they are marked, being two consecutive letters of the sentence "*Fortunam ex aliis.*"

Early in 1817, Dr Young having occasion to visit a patient in Paris, was greatly pleased with his reception in the scientific circles of that metropolis. With the Baron Alexander Von Humboldt, Messrs Arago, Cuvier, Biot, and Guy-Lussac, he had made previous acquaintance in this country. He found himself happy in renewing his intercourse with these very eminent men, and after his return to London, he went back to Paris for a few weeks in the summer of the same year.

In 1818, he was appointed by a commission under the Privy Seal, together with Sir Joseph Banks, Sir George Clerk, Mr. Davies Gilbert, Dr Wollaston, and Captain Kater, a commissioner for taking into consideration the state of the weights and measures employed throughout Great Britain. Dr Young acted as secretary at the meetings of this Board, and to the three Reports which were laid before Parliament he furnished both the scientific calculations, and the attached account of the various measures customarily in use.

Towards the end of the year 1818, Dr Young was appointed secretary to the Board of Longitude, with the charge of the supervision of the Nautical Almanack, under a new Act of Parliament. This appointment was to him a very desirable one, though the labour in which it involved him was great, as his anxiety to increase his medical practice henceforth ceased, and it made that the business of his life which had always been his inclination.

He now discontinued his residence at Worthing, and devoted

ed the summer to a hasty tour into Italy, an object which he always had in view. In about five months he visited all the most remarkable Italian cities, and amongst other objects of interest, gave the first place to the examination of the Egyptian monuments preserved in that country. He returned to England by Switzerland and the Rhine.

From the year 1820 to the end of his life, Dr Young continued to furnish a variety of astronomical and nautical collections to Brande's *Journal*, the greater part of which were original, and others which were translated were accompanied by his own comments.

In 1821, he published anonymously an "*Elementary Illustration of the Celestial Mechanics of La Place, with some additions relating to the motions of Waves and of Sound, and to the cohesion of Fluids.*" This volume, and the article "Tides," in the *Supplement to the Encyclopædia Britannica*, were considered by him to have contained the most fortunate of the results of his mathematical labours.

The next year he went again to Paris, and in 1823 he published his "*Account of some recent Discoveries in Hieroglyphical Literature and Egyptian Antiquities,*" in which he gave his own original alphabet, his translation from papyri, and the extensions which that alphabet had received from M. Champollion. This was the first non-professional publication since 1804, to which he had prefixed his name, and made open claim to his discoveries; having, as stated in his Preface, now attained his fiftieth year, and having at last determined to throw off the shackles by which he had hitherto considered himself to be bound by the etiquette of a medical practitioner.

At this time he attempted to form a society of about fifty subscribers, for the lithography of a collection of plates of Egyptian antiquities, subservient to the study of hieroglyphical literature. This work was, however, entirely carried on by Dr Young, and was afterwards made over to the Royal Society of Literature, and continued during the remainder of his life, to be executed under his supervision.

In 1824, he made an excursion to Spa and to Holland, and on his return undertook the medical responsibility and mathematical direction of a society for life insurance. This was esta-

blished at a moment when a mania for joint stock companies was springing up in England, and which at the time was supposed to offer great pecuniary advantages; but Dr Young's most scrupulous regard to what he supposed the strictest justice, never forsook him: he declined all participation in the speculation, and confined himself to the performance of the duties which he undertook. The connexion, however, with this company led him into new lines of research, in which he took great interest. He contributed to the Royal Society a "Formula for expressing the Decrement of Human Life," which was published in the *Phil. Trans.* for 1826; and a "Practical Application of the Doctrine of Chances," to *Brandé's Journal* for October in the same year; whilst he had a singular satisfaction in witnessing the prosperity of the concern in the department under his direction.

The year before this he removed from Welbeck Street to a house which he had built in Park Square in the Regent's Park, where he continued to reside during the remainder of his life, and where, in a situation to which he was extremely attached, he led the life of a philosopher, surrounded by every domestic comfort, and enjoying the pleasures of an extensive and cultivated society, who knew how to appreciate him. He expressed himself as having now attained all the main objects which he had looked forward to in life as the subject either of his hopes or his wishes. This end being, to use his own words, "the pursuit of such fame as he valued, or of such acquirements as he might think to deserve it."

In 1827, Dr Young was elected one of the eight foreign members of the Royal Institute of France, and was much gratified, not only by the honour conferred, but by being associated with so many distinguished persons, with whom he had long been in habits of correspondence and of friendship.

His health had hitherto, with the exception of the consumptive tendency which had visited him in youth, been uninterrupted by a day's serious illness, and no person would have appeared as giving a promise of greater longevity; but in 1828 there was a perceptible diminution of strength. In that summer he went to Geneva, and appeared to suffer what was to him an unusual degree of fatigue on great bodily exertion, and

his friends from that time could not help remarking symptoms of age which appeared to be on the increase, and which contrasted strongly with the singular freedom from complaint which he had hitherto enjoyed.

During the time that Dr Young was abroad, the general state of the finances of the country had been submitted to the examination of a Committee of the House of Commons. Amongst other things, some of the severer economists had brought under their consideration the construction and utility of the Board of Longitude as being under the direction of the Admiralty, and as giving an allowance of a hundred pounds a year to certain professors of the two universities, whose attendance was not often called for. The committee did not consist of members who were much acquainted with science; not one scientific person was examined before them. The amount of saving by the abolition of the only salaries which the government of England held forth for the encouragement of science, little if at all exceeded L. 500 a year; and though many projects which might not prove of utility were referred by government to this Board, yet the sums actually expended through them on such as they might conceive to be useful, had been extremely limited. But on the recommendation of this committee a bill was passed abolishing the Board, at the same time permitting the Admiralty to retain the officer entrusted with the calculations of the *Nautical Almanack*.

Dr Young continued to execute these duties; but this singular, and as it should seem ill-advised proceeding, caused great heart-burnings and discontent in the scientific bodies, amongst those who considered themselves or their friends treated unhandsomely as well as illiberally, in the manner in which their services had been dispensed with; and the assistance of men of science was soon found to be so indispensable to many departments connected with the Admiralty, that a new council of three members, consisting of Dr Young, Captain Sabine, and Mr Faraday, was appointed for the performance of the duties which had before devolved upon the Board.

The discussions incident to this subject, and the various reports which Dr Young had in consequence to draw up, together involved him in more labour than the situation of his

health rendered him competent to perform without injury, and exacerbated a complaint which it afterwards appeared must have been long in progress, but which now was bringing him rapidly to a state of extreme debility.

He had from the month of February 1829, suffered from what he considered repeated attacks of asthma, and though he said little of it, as unwilling to alarm those about him, was evidently uneasy at the situation of his health. This gradually deteriorated. He had in the beginning of April great difficulty in breathing, with some discharge of blood habitually from the lungs, and was in a state of great weakness. His friends and physicians, Doctors Nevinson and Chambers, considered that there was something extremely wrong in the action of the heart, as well as that the lungs were very seriously affected.

Though thus under the pressure of severe illness, nothing could be more striking than the entire calmness and composure of his mind, or could surpass the kindness of his affections to all around him. He said that he had completed all the works on which he was engaged, with the exception of the rudiments of an Egyptian Dictionary, which he had brought near to its completion, and which he was extremely anxious to be able to finish. It was then in the hands of the lithographers, and he not only continued to give directions concerning it, but laboured at it with a pencil when, confined to his bed, he was unable to hold a pen. To a friend who expostulated with him on the danger of fatiguing himself, he replied it was no fatigue, but a great amusement to him.

In the very last stage of his complaint, in the last lengthened interview with the writer of the present memoir, his perfect self-possession was displayed in the most remarkable manner. After some information concerning his affairs, and some instructions concerning the hieroglyphical papers in his hands, he said that, perfectly aware of his situation, he had taken the sacraments of the church on the day preceding; that whether he should ever partially recover, or whether he were rapidly taken off, he could patiently and contentedly await the issue: that he thought he had exerted his faculties through life as far as they were capable of; but that for the last eight years he had been careful of straining them to more than he thought they

could compass without injury ; that he had settled all his concerns ; that if his health had been continued to him, he might have looked forward to the prolongation of much that was to be enjoyed ; but that though he was in no other suffering than that of great oppression and weakness, still that if life were continued in the state he then was of inability to any of his accustomed employments, he could hardly wish it to be long protracted.

His illness continued with some slight variations, but he was gradually sinking into greater and greater weakness till the morning of the 10th of May, when he expired without a struggle, having hardly completed his fifty-sixth year. The disease proved to be an ossification of the aorta, which must have been in progress for many years, and every appearance indicated an advance of age, not brought on probably by the natural courses of time, nor even by constitutional formation, but by unwearied and incessant labour of the mind from the earliest days of infancy. His remains were deposited in the vault of his wife's family in the Church of Farnborough in Kent.

To delineate adequately the character of Dr Young would require an ability in some proportion to his own, and must be ill supplied by one incompetent to judge of the talents of a man, who as a physician, a linguist, an antiquary, a mathematician, scholar, and philosopher, in their most difficult and abstruse investigations, has added to almost every department of human knowledge that which will be remembered to after times—" who," as was justly observed by Mr. Davies Gilbert, in his eloquent address to the Royal Society, over which he so worthily presided, " came into the world with a confidence in his own talents growing out of an expectation of excellence entertained in common by all his friends, which expectation was more than realized in the progress of his future life. The multiplied objects which he pursued were carried to such an extent, that each might have been supposed to have exclusively occupied the full powers of his mind ; knowledge in the abstract, the most enlarged generalizations, and the most minute and intricate details, were equally effected by him ; but he had most pleasure in that which appeared to be most difficult of investigation." The president added, that " the example is only to be followed by those of equal capacity and equal perseverance ;

and rather recommends the concentration of research within the limits of some defined portion of science, than the endeavour to embrace the whole."

Dr Young's opinion was, that it was probably, most advantageous to mankind, that the researches of some inquirers should be concentrated within a given compass, but that others should pass more rapidly through a wider range—that the faculties of the mind were more exercised, and probably rendered stronger, by going beyond the rudiments, and overcoming the great elementary difficulties, of a variety of studies, than by employing the same number of hours in any one pursuit—that the doctrine of the division of labour, however applicable to material product, was not so to intellect, and that it went to reduce the dignity of man in the scale of rational existences. He thought it so impossible to foresee the capabilities of improvement in any science, so much of accident having led to the most important discoveries, that no man could say what might be the comparative advantage of any one study rather than of another; and though he would scarcely have recommended the plan of his own as the model of those of others, he still was satisfied in the course which he had pursued.

It has been said, that the powers of imagination were the only ones of which he was destitute. From the highly poetical cast of some of his early Greek translations, this is at least doubtful. It might, perhaps, have been said more justly, that he never cultivated the talent of throwing a brilliancy on objects which he had not ascertained to belong to them. Dr Young was emphatically a man of truth. The truth, the whole truth, and nothing but the truth, was the end at which he aimed in all his investigations, and he could not bear, in the most common conversation, the slightest degree of exaggeration, or even of colouring. Now, all exercise of what is ordinarily called imagination, is the figuring forth something which, either in kind or in degree, is not in truth existent; and whether originally gifted with this faculty, or otherwise, Dr Young would, on principle, have abstained from its indulgence.

To sum up the whole with that which passes all acquirement, Dr Young was a man in all the relations of life, upright, kind-

hearted, blameless. His domestic virtues were as exemplary as his talents were great. He was entirely free from either envy or jealousy, and the assistance which he gave to others engaged in the same lines of research with himself, was constant and unbounded. His morality through life had been pure, though unostentatious. His religious sentiments were by himself stated to be liberal, though orthodox. He had extensively studied the Scriptures, of which the precepts were deeply impressed upon his mind from his earliest years; and he evidenced the faith which he professed, in an unbending course of usefulness and rectitude.

* * This article is abridged from a Memoir of Dr Young's Life, London, 1831, drawn up from some short memoranda of his own writing in the possession of a near connexion. The Reverend G. Peacock is, we understand, engaged in an elaborate life of this distinguished individual.

ART. II.—*Errors in the Nautical Almanac for the year 1832; and in the Planetary Ephemerides for 1832 and 1833.*

To the Editor of the Edinburgh Journal of Science.

SIR,

A very considerable number of errors having been detected in the above works, and it being of importance that the fact of their existence should be published as extensively as possible, I shall feel obliged if you will permit me, through the medium of your *Journal*, to announce, that printed lists of the errors, with their corrections, may be had (*gratis*) by applying to the publisher, Mr Murray of Albemarle Street.—I am, Sir, your obedient servant,
 W. S. STRATFORD,
 Dec. 29, 1831. Superintendent of the Nautical Almanac.

Errata detected in the following Tables :—

I.—Nouvelles Tables Astronomiques et Hydrographiques, &c.
 par V. BAGAY. Edition Stéréotype. Paris, 1829. 4to.
 (Communicated by M. Bagay.)

NEW SERIES, VOL. VI. NO. II. APRIL 1832.

o

1st, Logarithmes des Nombres.—Table XXIII.

Page 44	Number 2301	for 36191.69 read 36191.66
46	3495	— 57344.72 — 54344.72
46	3595	— 55559.89 — 55569.89
50	5962	— 74539.20 — 77539.20
54	8165	— 91195.32 — 91195.62
59	11385	— 05533.30 — 05633.30
67	16084	— 20636.41 — 20639.41
72	18664	— 77100.47 — 27100.47
72	18868	— 24572.59 — 27572.59
75	20415	— 30994.14 — 30994.94

2d, Table des Logarithmes des Sinus, Cosinus, &c.

Page { 60 } { 70 }	In the <i>seconds</i> column, on the right hand, between 35 and 37, for 56 read 36.		
104	Cosinus.	4° 41' 0"	for 8.998 read 9.998
106 } 148 }	In the <i>seconds</i> column, on the right hand, between 35 and 37, for 56 read 36.		
125	Tang.	5° 37' 60"	for 40434 read 40454
189	Tang.	9 27 32	— 6883 — 6882
197	Tang.	10. 6 23	Insert the sign indicating change of preceding figure at the head of the column.
230	Sinus.	12 58 12	for 1018 read 1019
231	Cotang.	12 28 10	— 6934 — 6937
241	Cotang.	13 47 5	— 2310 — 2301
248	Sinus.	14 36 48	— 0334 — 0335
261	Tang.	15 22 0	— 9.939 — 9.439
265	Cotang.	15 41 7	— 5875 — 5873
266	Cosinus.	15 54 23	— 0446 — 0445
321	Tang.	20 28 8	— 0183 — 0184
355	Cotang.	23 14 5	— 2206 — 2208
355	Tang.	23 18 10	— 2000 — 2006
355	Cotang.	23 17 24	— 0666 — 0660
357	Cotang.	23 23 31	— 9418 — 9419
366	Sinus.	24 17 53	— 3520 — 3523
366	Sinus.	24 18 36	— 5523 — 5528
389	Cotang.	26 4 58	— 2276 — 2275
392	Sinus.	26 25 0 at bot.	— 35' — 34'
401	Cotang.	27 8 6	— 3087 — 3088
403	Cosinus.	27 15 15	— 8936 — 8939
403	Cotang.	27 19 18	— 8300 — 8309

Page. 424	Cosinus.	29° 0' 60"	for 9941 read 9.941
425	Cotang.	29 2 7	— 6177 — 6176
435	Cotang.	29 53 10	— 5665 — 5565
448	Sinus.	31 5 45	— 0469 — 0460
467	Cotang.	32 34 20	— 6046 — 6053
510	Cosinus.	36 13 51	— 6816 — 6810
529	Tang.	37 42 48	— 3255 — 3253
529	Tang.	37 46 26	— 2726 — 2736
531	at the bottom.		— Tang. — Cotang.
531			— Cotang. — Tang.
554	Sinus.	39 58 48	— 8863 — 8868
558	Sinus.	40 15 22	— 3705 — 3706
586	Sinus.	42 35 21	— 4193 — 4198
586	Sinus.	42 33 23	— 1498 — 1493
590	Cosinus.	42 53 0	— 9864 — 9.864

II.—Astronomical Tables and Formulæ, &c. By FRANCIS BAILY, Esq. London, 1827 and 9. 8vo.

Page 130, Table IV. transpose the 1st and 5th columns; and to the Equations corresponding to Arguments 0 to 500, annex the sign —, and 500 to 1000 the sign +

In the Appendix, published January 1829.

P. 270, line 23, for $0^s,177 + y \times 0^s,0084$, read $0^s,190 + y \times 0^s,0084 + y^2 \times 0^s,00000815$.

“ This includes the secular Equation of the Equinoxes, and also corrects a slight error in the Epoch, in page 196, but still leaves the mean daily motion of the Sun in Right Ascension, in Table II. equal to $3^m 56^s,555333$, instead of $3^m 56^s,555347$, as assumed in the Computations of the Nautical Almanac for 1834, now in progress.”

III.—Tables Astronomiques publiées par le Bureau des Longitude de France. Tables de la Lune. Par M. BURCKHARDT. Paris, December 1812.

Introduct. p. 3, l. 6, for du nœud $4^s 14^o 10' 1''$, 2 read $4^s 14^o 10' 12''$, June 10, Argument 6, for 5528 read 9528.

24, for May 26 | 23 read 26 | 25

74, Lat. 90^o , Rapport des Axes 329 : 330 for 9.9986620 read 9.9986820

IV.—TABLES Portatives de Logarithmes, &c. Par FRANCOIS CALLET. Edition Stéréotype. (Communicated by M. Bagay.)

1st, Errata corrected in the TIRAGE of 1825.

2 10 35	Sinus.	for 8.5795294	read 8.5795094
2 39 23	Sinus.	— 8.6660184	— 8.6660134
3 12 43	Tang.	— 8.7491027	— 8.7491007
3 34 20	Tang.	— 8.7953491	— 8.7953791
3 37 16	Tang.	— 8.8012780	— 8.8012980
3 38 8	Sinus.	— 8.8020567	— 8.8021567
4 43 39	Sinus.	— 8.9150160	— 8.9160160
4 51 14	Tang.	— 8.9280079	— 8.9290079

2nd.—Errata corrected in the TIRAGE of 1829.

0 24 54	Tang.	for 7.8599831	read 7.8599331
2 34 53	Sinus.	— 8.6535829	— 8.6535839
3 31 7	Tang.	— 8.7887952	— 8.7887951
4 34 17	Sinus.	— 8.9014641	— 8.9014647
4 37 38	Tang.	— 8.9081432	— 8.9081433
4 38 1	Sinus.	— 8.9073245	— 8.9073234
4 53 55	Tang.	— 8.9330113	— 8.9330103

V.—Ephemeris of the Distances of the Four Planets, Venus, Mars, Jupiter, and Saturn, from the Moon's Centre; together with their Places for every Day in the Year 1832, &c. Calculated under the direction of H. C. SCHUMACHER. Copenhagen, 1830. 8vo.

Page 14 March 22, IX ^h	for	50 49 11	read	50 29 11
14 May 3, Noon,	—	103 23 42	—	104 23 42
14 ——— III	—	105 7 51	—	106 7 51
14 ——— VI ^h	—	106 51 57	—	107 51 57
14 ——— IX ^h	—	108 35 59	—	109 35 59
29 June 10, Midnight,	—	122 20 30	—	122 22 30
32 October 31, IX ^h	—	38 12 8	—	38 22 8
44 October 12, III ^h	—	117 51 29	—	117 52 29
44 ——— VI ^h	—	116 9 24	—	116 10 24
44 ——— IX ^h	—	114 27 9	—	114 28 9
55 July 22, Declination,	—	21 26 33	—	21 26 23
56 Aug. 16, Right Ascens.	—	40 ^h	—	10 ^h

Page 62	Feb. 21, Right Ascens. for	18 ^h	read	19 ^h
67	Mars, July 26 to end of the Year, Log. Dist. for 0 in the	Index read 9.		
68	August 24, Latitude. —	2 3 22	—	2 2 22
73	January 31, Longitude, —	329 15 23	—	329 15 33
90	June 23, Declination, —	9 41 19	—	9 40 19
91	Saturn, July 14 to November 8, Log. Dist. for 0 in the	Index read 1.		

VI.—Ephemeris of the Distances of the Four Planets, Venus, Mars, Jupiter, and Saturn, from the Moon's Centre; together with their Places, for every Day in the Year 1833, &c. Calculated under the direction of H. C. SCHUMACHER. London 1831.

Page 3	head of the column, for XX ^h	read XXI ^h
6	July 10, Noon, —	41° 39' 8" — 41° 39' 38"
7	in column of dates, —	June 28 — June 23
9	September 17, XXI ^h —	105° 5' 15" — 100° 5' 15"
14	January 27, I ^h —	35° 3' 15" — 35° 8' 15"
17	line 11, in col. of dates, —	May — April
17	line 12, XXI ^h —	38° 44' 29" — 88° 44' 29"
18	May 31, IX ^h —	69° 3' 39" — 109° 3' 39"
19	line 14, in col. of dates, —	June 24 — June 14
21	September 11, XV ^h —	41° 51' 53" — 41° 51' 35"
27	May 11, XV ^h —	56° 55' 33" — 56° 25' 33"
27	May 24, XXI ^h —	115° 34' 19" — 115° 34' 9"
39	March 27, XXI ^h —	83° 39' 20" — 83° 89' 0"
39	April 7, XV ^h —	70° 76' 33" — 70° 26' 33"
40	July 26, VI ^h —	73° 45' 0" — 73° 45' 10"
43	November 5, XV ^h —	38° 28' 39" — 38° 28' 59"
46	Jan. 22, Log. Dist. —	0.95266 — 0.05266
51	June 2, Longitude —	52° 42' 4" — 54° 42' 4"
52	Right Asc. at the head —	° ' " — h m s
56	Nov. 30, Right Asc. —	17 ^h 39 ^m 9 ^s ,2 — 17 ^h 38 ^m 9 ^s ,2
56	{ Nov. 1 } Declination	Insert S
	{ — 30 }	
58	Jan. 10, Declination, —	13° 50' 57" — 12° 50' 57"
60	March 5, Log. Dist. —	6.85656 — 9.85656
63	June 12, Right Asc. —	35 ^h 6 ^m 20 ^s ,2 — 3 ^h 6 ^m 20 ^s ,2
66	Sept. 20, — — — — —	9 ^h 42 ^m 37 ^s ,7 — 9 ^h 22 ^m 37 ^s ,7
68	Nov. 3, — — — — —	12 ^h 33 ^m 53 ^s ,9 — 12 ^h 43 ^m 53 ^s ,9

Page 69	Dec. 19, Log. Dist.	for 0.19897	read 0.19879
70	Jan. 24 to 31, ———	— 0 in the index —	9
71	Feb. 7, Declination,	— 23° 28' 37" —	22° 28' 37"
73	April 20, Log. Dist.	— 6.24665 —	0.24665
74	May 1, ———	— 0.27055 —	0.27005
74	May 4, Declination	— 34° 13' 48" —	24° 13' 48"
74	May 31, Right Asc.	— 3 ^h 13 ^m 24 ^s .8 —	8 ^h 13 ^m 24 ^s .8
79	Oct. 31, Declination	— 14° 5' 6" —	14° 5' 16"
82	Jan. 26, Right Asc.	— 23 ^h 50 ^m 23 ^s .6 —	23 ^h 50 ^m 28.6
85	April 20, Log. Dist.	— 6.77222 —	0.77222
87	June 18, Latitude	— 1° 10' 53" —	1° 10' 55"
88	July 19, Log. Dist.	— 0.68651 —	0.69651
88	July 26, ———	— 0.98710 —	0.68710
89	Aug. 7, ———	— 6.67053 —	0.67053
98	May 31, Declination	— 4° 35' 10" —	5° 35' 10"
99	June 7, Log. Dist.	— 6.97029 —	0.97029
110	May 11, Longitude	— 322° 26' 46" —	322° 27' 46"
113	Aug. 25, Right Asc.	— 21 ^h 30 ^m 7 ^s .8 —	21 ^h 31 ^m 7 ^s .8
113	Aug. 31, Declination	— 21° 34' 31" —	15° 34' 31"
114	Sept. 5, Right Ascens.	— 21 ^h 27 ^m 2 ^s .3 —	21 ^h 27 ^m 2 ^s .8

VII.—Tables of Logarithms, &c. By MICHAEL TAYLOR.
London, 1792. 4to. (Communicated by M. Bagay.)

1° 18' 5"	Cotangent	for 36407	read 36408
4 38 39	Sine	— 82095 —	83095
4 46 19	Cosine	— 84921 —	84920
5 18 6	Cotangent	— 24681 —	24682
5 52 18	Cotangent	— 78371 —	78372
6 17 48	Sine	— 00135 —	01135
6 18 49	Cotangent	— 60810 —	60820
6 30 31	Cotangent	— 27605 —	27606
8 22 42	Cosine	— 3400 —	3401
9 22 4	Sine	— 5722 —	5773
9 57 18	Cosine	— 4115 —	4114
9 57 19	Cosine	— 4110 —	4111
12 55 29	Cosine	— 8552 —	8553
13 51 52	Cosine	— 1591 —	1590
14 2 8	Tangent	— 9177 —	9178
14 2 8	Cotangent	— 0823 —	0822
14 2 9	Tangent	— 9267 —	9268
14 2 9	Cotangent	— 0733 —	0732

14° 18' 2"	Cosine	for	3398	read	3298
15 21 49	Cosine	—	1960	—	1959
15 28 24	Cotangent	—	8072	—	7972
18 2 43	Sine	—	0373	—	0372
19 15 25	Tangent	—	2638	—	2628
19 15 25	Cotangent	—	7362	—	7372
19 15 26	Tangent	—	2705	—	2696
19 15 26	Cotangent	—	7295	—	7304
19 15 27	Tangent	—	2773	—	2763
19 15 27	Cotangent	—	7227	—	7237
19 15 28	Tangent	—	2841	—	2831
19 15 28	Cotangent	—	7159	—	7169
19 15 29	Tangent	—	2908	—	2898
19 15 29	Cotangent	—	7092	—	7102
19 24 59	Cotangent	—	8691	—	8690
19 36 51	Sine	—	9312	—	9313
20 36 39	Cotangent	—	7096	—	7069
21 9 42	Tangent	—	8286	—	8287
21 9 42	Cotangent	—	1714	—	1713
22 46 9	Tangent	—	9682	—	9681
22 46 9	Cotangent	—	0318	—	0319
22 46 10	Tangent	—	9741	—	9740
22 46 10	Cotangent	—	0259	—	0260
22 46 11	Tangent	—	9800	—	9799
22 46 11	Cotangent	—	0200	—	0201
22 46 12	Tangent	—	9859	—	9858
22 46 12	Cotangent	—	0141	—	0142
22 46 13	Tangent	—	9918	—	9917
22 46 13	Cotangent	—	0082	—	0083
22 46 14	Tangent	—	9977	—	9976
22 46 14	Cotangent	—	0023	—	0024
27 10 53	Sine	—	7347	—	7346
27 10 54	Sine	—	7388	—	7387
27 10 55	Sine	—	7429	—	7428
27 10 56	Sine	—	7470	—	7469
27 10 57	Sine	—	7511	—	7510
27 10 58	Sine	—	7552	—	7551
27 10 59	Sine	—	7593	—	7592
27 23 48	Cosine	—	3357	—	3358
29 29 49	Cosine	—	7098	—	7099
29 55 36	Cosine	—	8510	—	8510
32 29 55	Sine	—	1000	—	2000

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35 47 31	Cotangent	for	0592	read	0593
39 29 45	Cosine	—	4320	—	4321
39 56 41	Sine	—	5676	—	5677

VIII.—The Nautical Almanac and Astronomical Ephemeris for 1832. 8vo.

P.95 The Sun, Aug. 29, III^h for 49°.42'.53" read 49°.32'.53"
 122 Nov. 7, R. Asc. — 44^h.50^m.33^s,1 — 14^h.50^m.33^s,1

IX.—Tables of the Logarithms of the Natural Numbers, &c. By CHARLES BABBAGE, Esq. Stereotyped. Second Edition. London, 1831. 8vo.

Page 199. In the logarithms of the Numbers 106611 to 106619, both inclusive, the change in the fourth figure is *not* indicated by using the *small* type in the place of the fifth figure.

ART. III.—*On the Want of Encouragement in Science and Literature.* By SIR NICHOLAS HARRIS NICOLAS, K. G. H.*

IN the preceding observations on the impediments which, with the sanction of the government, exist to the extension of historical knowledge, all which has been asked for its advancement is, that the pure streams of information may be no longer dammed up, but that all those who wish to quench their thirst for historical and antiquarian inquiries, may be allowed to do so without expence. But this is not all which might be expected from a liberal and enlightened administration, though it is the most important boon which it can grant, and without which all other assistance would be useless.

It is proper to inquire what the inducements are in this country for a man to devote his life to science, or to the higher branches of literature, of which branches history is undoubtedly entitled to the first rank.

The ordinary motives which influence a man on embracing any pursuit or profession, besides the love of fame, are a wish for rank and honours, and more generally a desire for money.

* From *Observations on the State of Historical Literature, &c.* London, 1830.

It is notorious that scientific or historical acquirements are not productive of pecuniary advantages, because scientific works rarely pay the expence of publication, and the demand is not greater for historical or antiquarian literature. Possibly Dr Lingard and Mr Sharon Turner may have derived a slight benefit from their engagements with their publishers, but the amount does not bear any proportion to their labours; and it may be doubted if they have received so much as a clever artisan would have earned in the same time. Mr Hallam's "*Constitutional History*" has not, it has been said, even paid its expences, and the same remark applies to nearly every work of an historical nature, which has been published for the last ten years.

If the proceeds of the sale of such works have, in a few instances, covered the costs of the publication, a second edition has very rarely been required, and little, if any, profit has accrued to their authors or editors. Even "*Pepys's Diary*," the most entertaining, and consequently the most likely to be popular of the class, is by no means a profitable speculation; and it is unquestionable that, at this moment, no remuneration whatever is to be derived from the publication of a standard historical book, by which is meant works containing letters or other historical evidence, or treating of any particular event in English history.

This circumstance alone is sufficient to explain why valuable works on history so rarely appear; but a still more serious impediment to the publication of historical researches remains to be stated.

Supposing that there are persons who can afford to devote themselves to pursuits so utterly profitless, in what way are they to give their labours to the world? Not a single publisher in London, at this moment, will risk the cost of paper and print upon a volume illustrative of history, however interesting or important it may be. Not very long since a translation of the journal of the ambassadors, who were sent to negotiate the marriage of Henry the Sixth with the daughter of the Count d'Armagnac, was offered to *six* of the most eminent publishers in the metropolis, and though the manuscript proved that every historian has materially erred on the subject of that negotiation,

and abounds in curious and valuable notices of the state of the arts and of society, both in France and in this country, in the middle of the fifteenth century, *not one of them would print it on any terms*. From this fact, and many similar ones might be stated, the conclusion to be drawn is, that the public are generally as indifferent to historical literature as they are to science; and that as publishers naturally will not print what does not sell, those who are desirous of promoting the knowledge of English history, must not only relinquish all hope of being remunerated for their labours, but they must also publish their researches at their own expence. Though there may be a few persons whose private fortunes admit of their bestowing their *time*, the number is limited indeed of those who can expend their *money* in this manner.

It is incontrovertible, that pecuniary advantages are not to be expected from publications on science or history, and a life spent on either will end as it began, in poverty and comparative obscurity. Nor does eminence in those studies lead, in this country, to honours or distinctions of any kind.

Admission into the Royal Society, or the Society of Antiquaries, is any thing but an honour. The latter will not only receive any person as a member, but the situations of President and Vice-Presidents, instead of being reserved for the most distinguished writers on history and antiquities, are, with the exception of Mr Hallam, filled by individuals who have no pretensions to a profound knowledge of either; and in the Royal Society, the number of its members who are distinguished for their scientific attainments is extremely limited. Upon the total exclusion of scientific and literary men from the honours of the country, some very able remarks have recently appeared in the *Quarterly Review*, the only defect in which is, that literature is thrown too far into the back ground. The superior pretensions of science are conceded, but that superiority is not so disproportionably great as to justify the cavalier manner in which the writer, who is evidently a *scientific* person, seems disposed to treat literature. As an instance of this partial feeling, the impolitic and iniquitous tax which obliges eleven copies of every work to be given to public libraries, is said not in any way to operate to the disadvantage of

literature, whereas every publisher, and every author of any experience, is aware that it acts in many instances as an obstacle to the production of standard books. It is no more pretended that the author of every novel, or of every book of travels, than that he who amuses himself with "the cups and balls of science," ought to receive a mark of honour; but the writer who devotes his life to the profitless study of history in either of its branches, or to any other unpopular, but important, subject, from the pure motive of wishing to increase the stock of knowledge, is surely as entitled to reward as he who from motives equally disinterested, applies himself to science. Deplorable as may be the present state of science in England, the state of literature is no less lamentable; and those who carefully examine the works which issue from the press, will find, that every subject must, to use the words of publishers themselves on these occasions, be treated "in a light and popular manner," which means, as much divested of what is abstruse or profound as possible. Hence an author, who may be capable of, and desirous of doing, better things, is driven by pecuniary considerations, arising possibly from the duty which he owes to his family, to write a book, not according to his standard of value and ability, but according to the standard of the public taste, as defined by its caterer the publisher. This is as degrading as injurious to literature, but what other resource is there for a man of superior scientific or historical acquirements, who has no private fortune? The claims of each class on the government are consequently equal, and to obtain the admission of those claims, the most distinguished scientific persons, and the most eminent authors, should make it one common cause to press their pretensions to a share of the honours and public rewards of the country, upon the attention of the crown and the administration.

It would be untrue to say that there are no examples of honours having been bestowed by the sovereign in reward of science or literature, for of the many *hundred* baronets and knights who have been made in the last fifty years, Sir Humphry Davy and Sir Walter Scott obtained the former rank from their talents, and a few scientific persons have been knighted. Knighthood, however, has in no instance been conferred *for li.*

terary merit, and, incredible as it may seem, Sir Walter Scott is the *only* example in England of an *author* having been distinguished by any title of honour, since the accession of George the Third.

Since that period, physicians without number have been knighted and made baronets, and knighthood has been bestowed upon architects, chemists, musicians, painters, merchants, tradesmen, and, in short, upon every class of the community excepting upon *literary men* for *literary merit*.

It is by no means contended that simple knighthood—the dignity which is offered to every tradesman who may become Lord Mayor or Sheriff of London,—is a suitable reward for scientific or literary eminence; and proper as it was to honour Sir James South with a mark of his Majesty's favour, the compliment lost nearly all its value from being bestowed, on the very same occasion, upon the Mayor of a provincial city on presenting an address, and upon one of the Sheriffs of London, neither of whom was in any way distinguished for his personal merits or services.

Every one must readily admit that it was highly proper to confer a mark of honour upon Sir James South, and the only objection which can be made to it is, that he deserved a higher dignity. But there are many other scientific persons whose pretensions are fully equal to those of that eminent individual, great as they undoubtedly are, and if they are to be passed over, the act in favour of one becomes invidious to all.

It cannot be said with any thing like truth, that literary eminence, purchased, as in many cases it must be, like scientific fame, by the devotion of time and money to pursuits that yield in importance to science only, and which like science produce no remuneration in return, is not also deserving of royal encouragement; but as yet, claims of this nature have been wholly overlooked.

Though the rank of knight bachelor has lost nearly all its value in consequence of the miscellaneous description of persons who receive it, and because it is very seldom tendered as the reward of merit, the dignity of baronet is not only valueless for the same reasons, but it would be improper to bestow it on philosophers and authors, because all *hereditary* honours,

unless accompanied by an ample fortune for their support, have the effect of creating the greatest nuisances which can infest a country, a race of titled persons without property, whose merits are not sufficient to buoy them up in the great ocean of society.

It is remarkable, that though the bar, the army, the navy, diplomacy, and wealth, have proved passports to the peerage, there is not a single example on record of that dignity being conferred as the reward of scientific or literary talents. It may be said, that though in theory the House of Lords requires the profoundest knowledge of *every description* for the public welfare, yet that experience has shown the fallacy of such an idea; and upon this ground only can the extraordinary fact be explained, that not one of the men with whose fame that of this country is identified, has been raised to the peerage. Were the legal and constitutional practice of occasionally creating peers *for life* again adopted, the House of Lords would probably be distinguished by having some of the great names of the age among its members.

When the tremendous influence which the press possesses in England is considered, the anomaly becomes extremely glaring, that literature should never have led to honours of any kind, or to a place in that branch of the legislature where talents would prove most beneficial to the community. One reason is, that in England there is no "esprit de corps" among literary or scientific men; and they have too often manifested a sycophantic deference to mere rank in the dedication of their works, and in the rules of scientific and literary societies, which rank has properly repaid by the most sovereign contumely. If a peer, and even a "son of a peer," finds that he may become a Fellow of the Royal Society by virtue of a special law, dispensing with conditions which are enforced against the first philosopher of the day, (see Note A.) can he draw any other conclusion, than that he *confers* honour upon the society by entering into it? Such a regulation is disgraceful in the extreme to the Royal, and any other scientific or literary society, and until this stain be removed from their characters, it is absurd to talk of the neglect with which the professors of science

and literature are treated by the government and the aristocracy.

The aristocracy of rank has hitherto, it is feared, been hostile to the admission of the claims of genius to a share of the honours of the state ; but there exists at this moment an aristocracy of talent, whose political power has not yet wholly developed itself. It is not too late for the government and the peerage to consider whether it would not be expedient to produce, as far as may now be possible, a community of interests between those two bodies, by admitting the latter to some of the advantages of the former.

Can it be denied that those who have promoted the interests of their country, and of the world in general, by their scientific discoveries, or instructed and enlightened mankind by their writings, should be rewarded by those distinctions which in Great Britain have been hitherto confined to particular descriptions of services, of which services some have been as honourable as others have been base ?

It is not, however, by the mere withholding of titles and marks of honour, that neglect or rather contempt for scientific and literary men has been manifested by the British government. They are carefully excluded from the direction of public institutions for the advancement of science and literature generally, as in the instances of trustees of the British Museum, or of particular departments of literature, as in the instance of the commissioners for the preservation of the records, and of the commission for printing part of the manuscripts in the State Paper Office. All that the Crown has ever done for the advancement of science and literature, were the acts of his late Majesty, who placed two medals at the disposal of the councils of the Royal Society, of the Society of Antiquarians, and of the Royal Society of Literature, and founded ten literary fellowships of the value of L. 100 per annum.

Upon these fellowships all which will be said is, that it is by no means certain, that they have been conferred upon the most deserving persons, and that it is a great abuse of the institution to allow an individual whose church preferment exceeds L. 1000 per annum to enjoy one of them. The medals can scarcely be deemed a proper manifestation of the Royal favour.

If his late Majesty (see Note B.) had chosen the most gratifying and effectual method to reward scientific and literary merit, the plan would have been, not to place medals (see Note C.) which were not intended to be worn, and which confer no rank or title of honour, at the disposal of a "council" of any society, but to have given distinctions with his own hands, similar to those which he bestowed on his army and navy, and on his courtiers and friends; namely, by admitting them to one of the orders of knighthood. (See Note D.)

It is not a little extraordinary that in every other country of Europe, science and literature, as well as military merit, are rewarded by honorary distinctions, (see Note E.) though the greater part of those states are *military*; yet in England, which is avowedly *not a military country*, civil merit has never been so distinguished.

Honours of that nature are reserved for peers with parliamentary influence, or who are personal friends of the sovereign, or for diplomatic, military, or naval services; and Sir Joseph Banks is the only individual in *any way* connected with science who has been admitted into either of the various orders of British knighthood.

At the close of the late war in 1814 the order of the Bath was very much extended. Instead of one class, it now consists of three classes, containing altogether not less than *seven hundred members*. The first class are called Knights Grand Crosses, and they are divided into *Civil* and military knights. But there are no *Civil* knights of the inferior grades, and the rank of *civil* Knight Grand Cross is bestowed in reward of diplomatic services *only*.

The present state of the order of the Bath presents, however, a favourable opportunity for conceding the claims of genius, unless his Majesty should prefer bestowing the various classes of his own *family* order of the Guelphs upon the more distinguished labourers in the fields of science and literature; an act which would be greatly enhanced by its being his own spontaneous deed, and, consequently, having the character of a *personal* mark of royal favour. The order of the Bath must be partially re-modelled, on account of the blunders and absurdities of the new regulations. Even the official instrument

for the alteration has never been executed, and the statutes have not been printed, though the new knights *paid* for their copies fifteen years ago, since which time many of them have died. If, therefore, it be at last thought proper to admit science, and literature, and art, as well as other civil services to the honours of the state, and thereby to concede the principle, that those who have promoted the happiness, the prosperity, and the knowledge of mankind, are as worthy of honours as those who have excelled in destroying their fellow creatures in war, (see Note F.) a proper opportunity will be afforded for the purpose.

By extending the *civil* knights grand crosses from twelve to twenty, and instituting thirty *civil* knights commanders, and perhaps fifty *civil* companions, the claims of science, literature, and art, would be fully satisfied. The additional civil knights and civil companions should consist of persons who are the most celebrated in those pursuits; but the utmost care ought to be taken in the selection; and scientific discoveries of importance, and works of unquestionable value, only, should entitle their authors to the distinction.

The higher class of the order ought to be approached through the class immediately beneath it, so that a stimulus would exist to future exertions. By a measure of this kind, those who have spent their days in rendering their fellow men wiser, more virtuous, and more useful, would be saved from having the privations of poverty increased by the most heart-rending of all feelings—the consciousness of unmerited neglect.

Wealth never has been, and probably never will be, the result of high intellectual labours, but marks of honour from the sovereign would secure respect to genius, and give dignity to poverty. Such an act would mark the reign of WILLIAM THE FOURTH with imperishable glory; for though his predecessors may have been aware of the *existence* of merit, the honour has been reserved for HIM TO REWARD it.

But it may be asked, Can no pecuniary advantages be bestowed upon those who, from having devoted their lives and fortunes to pursuits which do not yield remuneration, require assistance from the country? Nothing can be more easy, and this too, without adding to the public expenditure.

There are numerous situations, the duties of which require only a few hours daily attendance, but to which salaries of from L. 500 to L. 1200 a year are attached. Let some of these be bestowed on men of science, and those literary persons whose works, though of high value, are not of that popular nature to produce profit to their authors.

To this it may be objected, that there is an obstacle. If these situations were given as the reward of talents, peers and proprietors of boroughs must, like other parents, take care of their own offspring; and rather than produce so heavy a calamity, it might be preferable to allow all the philosophers, mathematicians, astronomers, historians, and other plebeians who have *nothing but their genius* to recommend them, to starve.

Unless something of the nature of the measures which have been adverted to be done for the encouragement of science and history, it is hopeless to expect that either will advance. The present state of both is humiliating to the nation; (see Note G.) and so long as men of talent find a profitable occupation in popular literature, and know that eminence in pursuits of a higher character is suffered by the State to be, like virtue, its own reward, so long in the common nature of things will those pursuits be neglected. (See Note H.)

The vital importance of science to a maritime nation like Great Britain is self-evident, nor is it likely to be denied that her history ought to be as accurate, as complete, and as satisfactory as existing materials can possibly render it.

For several centuries there was an historiographer royal, but the office was abolished from motives of economy, since which time *booksellers have been the only patrons of historical literature in England*. The taste of the age has now, however, destroyed that patronage, limited and humble as it was; and those who, in despite of the thousand obstacles by which historical inquiries are impeded, may be induced from a natural ardour which poverty cannot chill, nor neglect diminish, to remove any of the falsehoods, supply the defects, or illustrate the obscurities with which the history of England abounds, are entitled to some encouragement from the great fountain of honour, for the same reason that scientific merits deserve them;—because there is no other reward, and because science and

history cannot be neglected by this country, and be fostered by all others without national disgrace.

Let it be hoped, that a new era has dawned on science and literature by the recent accession, and by the late change of His Majesty's ministers; and that the stigma upon the national character, that England is the only country in Europe in which genius is excluded from the honours of the State, will be wiped away. The sovereign by whom, as well as the ministers, through whose advice the important objects advocated in Mr Babbage's valuable work, and in this volume, may be accomplished, will be sure of immortal fame, because honours and rewards will be bestowed upon those who can commemorate their benefactors to the latest posterity, in the name of a constellation or a metal, or in the unfading pages of HISTORIANS, those great dispensers of posthumous glory:

“ Vixere fortes ante Agamemnona
Multi: sed omnes illacrymabiles
Urgentur, ignotique longa
Nocte, carent quia vate sacro.”

NOTES.

(Note A.)—The statute alluded to is as follows, and it is extraordinary that no notice is taken of it by Mr Babbage, by Sir James South, or by the author of the pamphlet, entitled, “*Science without a Head*,” in their criticisms on the Royal Society. The latter writer adverts to the statute itself, but he is silent on the exception in favour of peers, whilst from some of his remarks it would almost seem that he *approved* of their being thus *entreated to honour the Society* by joining it. Does Mr Babbage, does Sir James South, think such a fawning deference to the aristocracy a proper characteristic of the Royal Society in the year 1830? Their silence would certainly be susceptible of such an inference, were it not that the idea of *their* approving of such a regulation is contradicted by the tone of honourable independence for which their publications are conspicuous.

“ Every person to be elected a Fellow of the Royal Society shall be proposed and recommended at a meeting of the Society, by three or more members, who shall then deliver to one of the Secretaries a paper signed by themselves, specifying the

name, rank, profession, and the usual place of residence of such person ; all which shall be certified from their personal acquaintance with him or with his writings.

“ A fair copy of which paper, with the date of the day when delivered, shall be fixed up in the common meeting room of the Society, at ten several ordinary meetings, before the said candidate shall be put to the vote. *Saving and excepting, that it shall be free for every one of his Majesty's subjects who is a PEER, or SON OF A PEER, of Great Britain or Ireland; and for every one of his Majesty's Privy Council, of either of the said kingdoms, and for every foreign sovereign prince, or the son of a sovereign prince, or an ambassador to the court of Great Britain,* to be proposed by any single member, and to be put to the vote for election on the same day, there being present twenty-one members at the least, being the competent number for making elections.” Chap. I. s. 111.

Upon a similar regulation in the Society of Antiquaries remarks have been made. It is proper to add, as an illustration of the propriety of these regulations, that when peers are elected on the council of either society, they rarely condescend to attend ; that of the *sixty-three temporal lords* in the Royal Society, *not one* has contributed even a single paper to its “ *Transactions* ;” and that of the *fifty-nine temporal lords* in the Society of Antiquaries, *one only* has written a paper for the “ *Archæologia*.” The criterion of merit adopted in estimating the scientific pretension of the Fellows of the Royal Society seems to be the *number* of their papers printed in the Society's “ *Transactions*,” but no allusion is made either by Mr Babbage or by the author of “ *Science without a Head*,” to a far more satisfactory criterion,—the *value* of those papers. To use *that word* in relation to the “ *Archæologia*,” with the exception of one or two volumes, would be ridiculous.

(Note B.)—As the sovereign is the fountain of honour, it is proper to attribute every thing connected with honorary distinctions to him in his individual capacity ; but there is reason to believe that his late Majesty was not only anxious to establish an order for *civil* merit, but that the arrangement was actually formed. His ministers, however, it is said, refused

their sanction, and the plan was abandoned. This refusal, when properly stated, means that individuals who had been well paid for every hour which they had devoted to the public, and who were themselves in the possession of high rank, refused to admit the claims of genius to a trifling but gratifying reward. As those personages could not be suspected of a community of feeling on the subject, their disapprobation was, however, what might be expected.

(Note C.)—Mr Babbage's remarks on the medals placed at the disposal of the Royal Society, may be consulted with advantage by those who consider that *medals* are of much utility in promoting science.—*Reflections on the Decline of Science in England*, p. 115—136. The value attached to royal medals by the Solons who govern the Society of Antiquaries has been pointed out.

(Note D.)—So desirous was his late Majesty of rewarding his *foreign* subjects in this manner, that he founded two Orders for the purpose, that of the Guelphs of Hanover, and the Order of St Michael and St George, for Malta and the Ionian Islands, into which the *civil* services of all kinds form grounds for admission. The Guelphic Order was most deservedly conferred upon Sir William Herschel.

(Note E.)—Numerous proofs of the patronage bestowed by the sovereigns of all other countries in Europe on men of science and authors are given in the article before referred to in the *Quarterly Review*. The case of Niebuhr, the celebrated historian of Rome, is another instance, and forms a striking contrast to the manner in which historians are treated in England. “The author of the *History of Rome* has received singular encouragement and extraordinary reward: he was appointed ambassador to the Holy See, not because the King of Prussia was likely to have many disputes with his holiness about the frontiers of their dominions, but the legation was created for the express purpose of enabling his excellency to enjoy advantages and facilities in pursuing his inquiries at Rome, which he could not have had in any other manner. On his return, to induce him to arrange his materials and make his views public, the professorship of history was founded for him in the University of Berlin. He was adorned so far as a monarch can

add to the honours of such a man, with many orders and other decorations; and as a farther recompence, that he might pursue his studies in an agreeable literary retirement, he was attached as a supernumerary under the name of a free associate to the University of Bonn; but

—'quæ munera fati
Acta viri pensare queant?

the means of rewarding literary merit are as ample in Great Britain as they are scanty in Prussia, where the government and the individuals are as notoriously and proverbially poor as they are opulent here; nevertheless, the writer of a work equally distinguished by solid learning, useful instruction, and profound and liberal views, would find that there was no embassy or other public mission ready to promote his researches, that no University would open her arms to receive him, still less would badges of honour be accumulated upon him."—*Edinburgh Review*, July 1830, p. 391.

(Note F.)—It is by no means intended by this expression to under-rate military and naval services, nor does the remark in the following eloquent passage in the *Quarterly Review* on the subject appear just, for success in battle, whether on the ocean or in the field, is not purchased by animal courage alone, but by professional skill, promptitude, and indomitable energy of mind.

"However desirable these changes would be under any circumstances, their influence would be limited, and their operation cramped, *unless our literary and scientific men are allowed like other ranks in society, to aspire to the honours of the State.* No statute indeed disqualifies them from holding the titles which reward the services of other men, but custom, as powerful as statute, has torn all such honours from their grasp, and while the mere possessor of animal courage, one of the most common qualities of our species, has been loaded with every variety of honour, the possessor of the highest endowments of the mind, he whom the Almighty has chosen to make known the laws and mysteries of his works, and he who has devoted his life, and sacrificed his health and the interests of his family in the most profound and ennobling pursuits, is allowed to live

in poverty and obscurity, and to sink into the grave without one mark of the affection and gratitude of his country."

(Note G.)—Although the author of "*Science without a Head*" denies that science has *declined* in England, even he admits that nearly every other state can boast of scientific pre-eminence over this country. His remarks on the effect of honours in promoting science may require refutation, when it is known how far *that writer* is an *authority* on such a subject.

(Note H.)—"Of scientific men, some waste their hours in the drudgery of private lecturing, while not a few are torn from the fascination of original research, and are compelled to waste their strength in the composition of treatises for periodical works and popular compilations."—*Quarterly Review*.

ART. IV.—*Instructions, Mathematical and Practical, on a Method for Polishing Concave Lenses and Specula, with certainty, to figures produced by the revolutions of any of the conic sections about their major axes.* By R. POTTER, Esq. Junior. Communicated by the Author.

IN my paper on the working of specula and lenses, published in a previous number of this *Journal*, I referred to a method of using the rotatory motion of the polishing lathe to reduce a spherical figure to another which approached much more nearly to an ellipsoidal, or a paraboloidal one, and which must of course be much more valuable in optical instruments.

At the time of my writing that paper, I had polished a speculum for the microscope of one inch diameter, and one and a half inch solar focal length, which I found by examination to have the same conjugate foci for its centre and its edge, when these foci were about twelve inches distant from each other; whereas, in a spherical mirror, the focus for the rays reflected near the edge is quite perceptibly shorter than that for those reflected near the centre in a mirror of such size and curvature. This experiment convinced me that the means of producing such a figure would be a great improvement in practical optics.

To this speculum I had given the figure by only keeping its centre, as nearly as possible during the working, over the

centre of revolution of the polishing tool, which was coated uniformly over with the mixture of pitch and rosin, as in other cases. The effect of the circular motion was consequently to wear away the metal more, as the distance from the centre was greater. I commenced with the figure as truly spherical as I could get it, and continued the rotatory polishing with putty for about four to five hours, and then finishing with the fine oxide of iron described in my paper above alluded to.

I found this speculum still to give a much sharper image with the rays reflected near its centre than with those reflected near its edge; but I considered that this might arise in a great measure from specula of this size, compared with their focal length, requiring a very exact adjustment, and which I had not the means of applying. I have now, however, no doubt, that, if I had examined any zones intermediate between the centre and the edge, I should have found a considerable difference in the foci. The fact of the image produced by the edge of the speculum being less distinct than that produced by the centre, induced me, nevertheless, to undertake a mathematical investigation of the nature of the curve which must result from such a method of working.

In the *first* place, we must assume that the lathe is only worked slowly, and that attention is paid that the polishing tool keeps in a uniformly effective state in all parts, so that they produce equal effects in abridging the metal by passing through equal spaces, whatever be their distance from the centre of revolution.

Let Figure 1, Plate II. represent a section of the lathe spindle with the polishing tool *a b c* screwed on its end, and *d e f* being a section of the speculum, with its conical handle cemented to it, placed as it remained during the process. We see that the effect of the rotatory polishing on the speculum (to prevent confusion of terms I continue to call it polishing, though it might with equal propriety be called grinding, whilst we use the common putty powder of the shops, on metals containing copper and tin without arsenic,) will be directly as the distance from the centre of the spindle, so that the further from the centre the greater will be the thickness worn away, the soft

nature of the pitch causing it by the pressure to keep the same figure as the speculum.

Let $a b$, Figure 2, be a circular arc, of which o is the centre, and $ao = r$ the radius; let the origin of the co-ordinates be in the point a , the line aox being the axis of x , and ay that of y . We have then the equation of the circular arc $y^2 = 2rx - x^2$. If now we consider any abscissa ap as diminished by a quantity worn away in polishing, which is equal to the corresponding ordinate (or distance from the centre of revolution,) multiplied by a constant quantity, we shall have the abscissa of the new curve ad for the same ordinate, equal to that of the circular arc, minus the quantity worn away; that is, $x' = x - my$, or $x = x' + my$. Substituting this value of x in the equation $y^2 = 2rx - x^2$, we have $y^2 = 2r(x' + my) - (x' + my)^2$, developing and bringing all the terms to one side, we have $y^2(1+m^2) + 2mxy + x'^2 - 2mry - 2rx' = 0$.

This equation rising only to the second degree, we know that it represents some one of the conic sections; and the values of the co-efficients of the three first terms inform us that this section is the ellipse. But the co-efficient of the second term not being zero, we know that the ellipse is referred to co-ordinates which are inclined at some angle to its axis depending on the value of m .

Removing, in the first place, the origin of the co-ordinates, without altering their direction, so as to get rid of the terms involving the first powers of x' and y , we find the new origin to be always in the axis of x , and in the point, which is the centre of the circular arc we commenced with; and the equation of the new curve considered in respect of this point, is, $y^2(1+m^2) + 2myx'' + x''^2 - r^2 = 0$.

We find the position of the axis of the ellipse which this equation represents to be such, that a being the angle which they form with the co-ordinate axis, we have tangent $2a = -\frac{2}{m}$, and as m will be always in practice a very small quantity, the angle will not differ sensibly from 45° , and the curve will be similar to ad , the points f and f' being the foci of the ellipse. We see at the first view that this curve can be only considered

at the best as a first approximation to the one we seek the means of executing.

In pursuit of these means, let us compare, first, the equations of the circle and parabola, considering them in a similar manner, and the circle being the osculating circle to the parabola at its vertex. Then knowing from the results of the higher calculus that the radius of curvature of all the conic sections at their vertex is equal to the semiparameter of the section, we have for the parabola the equation $y^2 = 2rx'$, when $y^2 = 2rx - x^2$ is that of the circle. For equal ordinates, or when y is equal to y , we have

$$2rx' = 2rx - x^2.$$

$$\text{and } x - x', \text{ or, as we shall write it, } \Delta x = \frac{x^2}{2r}.$$

The mirrors of a parabolic figure, or, speaking more correctly, of a paraboloid of revolution, are principally wanted for the large mirrors of reflecting telescopes, and have always very small depth of curvature, so that in the equation of the circle we may consider x as very small compared with $2r$, and of course x^2 as very small compared with $2rx$. Neglecting therefore x^2 before $2rx$,

$$\text{we have } x = \frac{y^2}{2r} \text{ very nearly.}$$

$$\text{and } \Delta x = y^4 \frac{1}{8r^3} \text{ for a very near approximation.}$$

Or the differences of the abscissæ of the two curves to the same ordinate, when the arcs are very small, are as the fourth powers of the ordinate; and thus these differences being expressed in a rational function of the ordinate, we shall find that there is no difficulty in finding an easy practical process for reducing a spherical to a paraboloidal mirror with a great degree of correctness.

There are two modes of applying practically the formula just found. In one of these we commence with a polisher consisting of a surface uniformly covered with the pitch mixture, and we remove the successive zones of it at the end of certain times, (which are as the third powers of the distances,) so as to produce an effect, which is as the fourth power of the distance from the centre of revolution. In this plan we take the time of the polisher working on any zone as the variable.

The other plan is to take the space upon the polishing surface as variable for different distances from the centre. The effect of the rotatory motion being, as we found before, directly as the distance from the centre, we find the arcs subtended of the effective polishing surface should be, at all distances, as the third powers of those distances. The whole resulting effect will be then as the fourth power of the distance which we found to be required.

We shall find, that, in the ellipse and hyperbola, the formulæ expressing the differences of the abscissæ for small arcs, form similar equations; that is, that the differences are for these curves also as the fourth powers of the ordinates, and that they differ only in the constant multiplier.

This constant quantity in practice depends on the time we continue the working, in such manner, that, on the last-named place, the spherical figure first becomes ellipsoidal, which gaining more and more eccentricity, at last becomes paraboloidal where one of the foci is at an infinite distance; and then, on continuing the process, one of the foci becomes negative, and the figure hyperboloidal.

The former of the methods was the one I first thought upon, and putting it into execution last summer, I formed by it ellipsoidal mirrors for the microscope, described in the last number of this *Journal*, and found them give an image infinitely superior to spherical ones of the same aperture and focus. I forwarded to Mr Tod, Secretary to the Society of Arts for Scotland, an essay, describing the principle and practical process, on the 16th July last. In October I first thought that the latter method would be much superior to the other. Other engagements have until lately prevented me from proving it; but having now executed specula of *equal diameter* (or aperture) to their *solar focal length*, of ellipsoidal figures with the distances very considerable between the conjugate foci, I can speak decidedly of the complete efficacy of the method, and of the correctness of the theoretical principles on which it is based. Hence I claim the title of first discoverer of the method of producing these curves on specula and lenses, which has been held so great a desideratum ever since Sir Isaac Newton executed the first reflecting telescope.

If any process, already published, can be shown to be adequate to the above purpose, I shall of course lose this title; and if any one can produce mirrors executed before last summer which will stand the test of severe examination, I must cede to him the claim of prior execution. As to the former point, I think there will be no difference of opinion, that there is no account yet in print of a mode of so working specula which is found sufficient in practice. The other point remains yet to be determined, though I have little fear of any speculum of such curvature as I have mentioned, and executed prior to mine, making its appearance for examination.

Having explained the theory of the process I follow, I shall now show the mode in which the formula for Δx is deduced for the ellipse and hyperbola, and then give tables of differences calculated by the formulæ, compared with the real differences obtained by subtracting the actual abscissa of the respective curves.

In the parabola we commenced with the supposed knowledge of its parameter, by means of which the equation of the curve is at once found; but in the ellipse and hyperbola we require to know the major and minor axes, as we have these curves of various excentricities to equal parameters. The semi-parameter being a third proportional to the semi-major and semi-minor axes of the section, we may establish such a connection between them that the radius of curvature at the vertex shall remain constant for a whole series of curves. Let A represent the semi-major and B the semi-minor axis of the section; then P being the semi-parameter, we have $A : B :: B : P$; and P being equal to r , the radius of the circular arc, we arrive at this equation of condition, that the circle may be the osculating one to the ellipse or hyperbola, namely $A r = B^2$.

Now the equation of the circle taken as before is $y^2 = 2 r x - x^2$, that of an ellipse similarly considered being $y^2 = \frac{B^2}{A^2} (2 A x' - x'^2)$

Establishing the condition that the circle is the osculating one at the vertex, we have $y^2 = \frac{A r}{A^2} (2 A x' - x'^2) = 2 r x' - \frac{r}{A} x'^2$.

When the ordinates are equal in the circle and the ellipse,

we have $2rx - x^2 = 2rx' - \frac{r}{A} x'^2$

$$\text{whence } x - x' \text{ or } \Delta x = \frac{x^2 - \frac{r}{A} x'^2}{2r}$$

Now when the mirror is small in comparison to its focal length, we may neglect, as before, x and x' before $2r$, in seeking an approximate equation. This is equivalent to considering $x' = x$; which it will be very nearly in small arcs; we have thus

$$\Delta x = x^2 \left(\frac{1 - \frac{r}{A}}{2r} \right) = \frac{y^4}{4r^2} \left(\frac{1 - \frac{r}{A}}{2r} \right) = y^4 \left(\frac{A - r}{8r^3 A} \right) \text{ very nearly.}$$

The equation of the hyperbola, considered as we have done those of the ellipse and parabola, being $y'^2 = \frac{B^2}{A^2} (2Ax' + x'^2)$ we find first

$$\Delta x = \frac{x^2 + \frac{r}{A} x'^2}{2r}$$

and then by substitution we have

$$\Delta x = x^2 \left(\frac{A + r}{2Ar} \right) = \frac{y^4}{4r^2} \left(\frac{A + r}{2Ar} \right) = y^4 \left(\frac{A + r}{8r^3 A} \right) \text{ very nearly.}$$

These equations, as will be seen, are of a similar form to that which we found for the parabola, and it will be seen in the tables that the values they give are very near approximations, except in cases where the size of the mirror (or arc) is enormous in respect of the focal length.

In the first table I have calculated the differences of the abscissæ for a paraboloidal speculum of 5 inches diameter and 50 inches focal length, which is considered a large proportion for the large mirror of a Newtonian telescope; and in the second I have calculated those for an ellipsoidal one of one inch diameter and one and a-half inches focal length, which, though not so large a proportion as I have lately worked, may still be called enormous.

Col. 1. Values of the ordinate in inches.

2. Versed sines of a circular arc to 100 inches radius.

Col. 3. Abscissæ of a parabolic arc to 100 inches semi-parameter.

4. Differences.

5. Differences given by the formula $\Delta x = y^4 \frac{1}{8r^3}$

1.	2.	3.	4.	5.
.5	.0012500078	.00125	.0000000078	.0000000078
1.0	.0050001250	.005	.0000001250	.0000001250
1.5	.0112506228	.01125	.0000006328	.0000006328
2.0	.0200020004	.02	.0000020004	.0000020000
2.5	.0312548843	.03125	.0000048843	.0000048828

For the ellipse, let it be required to determine the differences for a circle of three inches radius, and an ellipse in which the distance of the further focus from the vertex of the mirror shall be exactly 16 inches. By the properties of the ellipse, and our equation of condition, we find the value of the semi-major axis or A to be 8.827586, &c. and of the semi-minor axis or B to be 5.146140, &c. Calculating with these continued to ten places of decimals, I find the abscissæ and differences to be as below.

Col. 1. Values of the ordinate in parts of an inch.

2. Versed sines of a circular arc to 3 inches radius.

3. Abscissæ of an elliptical arc to the above radius for its axis.

4. Differences.

5. Differences given by the formula $\Delta x = y^4 \frac{A - r}{8r^3 A}$

1.	2.	3.	4.	5.
.1	.00166713	.00166682	.0000003	.0000003
.2	.0066740	.00666918	.0000048	.0000048
.3	.0150376	.01501276	.0000249	.0000247
.4	.0267862	.0267070	.0000792	.0000782
.5	.0419601	.0417654	.0001947	.0001910

We see from the above tables that the formulæ are quite sufficiently accurate for all moderate proportions in specula; nevertheless in the extreme sizes it will be advisable in practice to make the required allowances, in forming the polishing tool,

from actual calculations. This will seldom, however, be necessary in the specula for the microscope or Gregorian telescope until the diameter or aperture is about equal to the focal length.

In returning to the practical application of the above theoretical deductions, I shall not enter any further into the method I first followed of taking the time of working as the variable; the second mode, or that of forming the polishing tool such as to produce the required effect, being at the same time much easier and much more accurate,

The plan I pursue in preparing the polisher is this, I take a tool similar to the one the metal has been ground upon, and covering it with a very hard mixture of $\frac{1}{4}$ th pitch and $\frac{3}{4}$ ths rosin, I bring its surface to exactly the same curvature with that of the spherical speculum about to be polished. I make it of exactly the same size also as the speculum, then laying upon its surface a thick paper diagram which had been previously marked with the proportions of the pitch requiring to be cut away, I transfer those marks upon the pitch, and cut away as accurately as possible with the sharp point of a pen-knife the portion which is indicated.

To prepare the diagram we may use the following method. Taking a circular disc of paper a little larger than the speculum, so as to allow for the curvature it acquires when pressed between the metal and the tool. We then divide the radius of this disc into ten equal parts similar to Figure 3, or, if the curvature is considerable, a small allowance will be required in the outer divisions, and draw ten circles through the points thus found. The principle upon which we proceed requires the breadth of the pitch surface on every one of these circles to be as the fourth power of its radius, or the arcs *in degrees* to be as the third powers. The radii being as 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, these arcs should be as 1, 8, 27, 64, 125, 216, 343, 512, 729, 1000. But if we divide the outer circle into six parts of 60° each, then the numbers of degrees which should be on each of the others to give the required breadths is found by this formula, namely, $\text{arc} = \frac{r^3}{r'^3} \times 60^\circ$, where r is the radius of the outer circle, and r' that of the smaller one.

We now find these arcs to be as in the table below ; and we should find on examination, that the absolute breadths they indicate on their respective circles are as the fourth powers of the radii.

Radii.	Cubes.	Arca.
1	1	0°. 4'
2	8	0.29
3	27	1.37
4	64	3.50
5	125	7.30
6	216	12.58
7	343	20.35
8	512	30.43
9	729	43.45
10	1000	60°. 0

We may now set off these arcs by means of a scale of chords, such as is found in common cases of instruments ; or we may follow the better plan of finding the lengths of the chords from a table of sines, and may then set them off very accurately from a fine scale of equal divisions. On referring to Figure 3 it will be seen at once how this is accomplished. The shaded parts on the disc represent the actual polishing surface, and the parts left white, that which is to be cut away.

In the course of the working, continual attention must be paid that the figure of the pitch does not materially alter, and when any alteration is perceived it must be immediately corrected.

I first polish the mirrors to as true a spherical figure as I can upon another polisher, and then proceed to give them the figure of the conic section proposed, by the rotatory motion, and when I have continued the rotatory working so long as to produce the required alteration of figure, I finish off the metals on a common polisher of very soft pitch (without rosin) by the rotatory mode still, and using the fine oxide of iron.

This last part of the operation requires some care and dexterity with mirrors which have any other figure than spherical. The best mode I have yet hit upon, is that of using a polisher of very soft pitch, and adding soap and very little water frequently ; the greatest care is necessary in not spoiling the

figure of the metal, and the finishing part should be continued no longer after the polish has attained that high black lustre which is necessary for very critical defining power. A few fine scratches, or other similar imperfections are of exceeding small detriment, compared with the general figure, and should never induce the operator to continue his work longer than otherwise necessary.

I have found a very great difference in the specula I have yet polished, as to the time it takes me to produce an ellipsoidal figure of any given degree of excentricity. The tenacity of the metal, and the sharpness of the polishing powder, combined with the speed the lathe is worked at, and above all, the pressure with which the speculum is held against the tool, tend to make a difference in the length of time necessary to produce the same figure on different mirrors.

I have found that the requisite quantity might be worn away at the edge of a mirror of one inch diameter and one inch solar focal length, to reduce it to an ellipsoidal figure with about ten to twelve inches between the foci, in about even one hour's working; but I have never then been well satisfied with its execution in the microscope.

In the best mirror of the above dimension and focus I have yet executed, I only used a very moderate pressure, and spent about three hours in that part of the polishing process. The result is a correctness in defining minute microscopic objects, such as I could not have anticipated, notwithstanding its enormous proportion.

These specula give images of opaque objects of a brilliancy which would surprise those who have been accustomed to work with common microscopes.

The effect is indeed great, compared with any thing I had before seen. Critical sharpness of the markings on small fibres and other objects, at the same time with a brightness which leaves the eye nothing to wish for, is the result of an accurate figure and large aperture.

I have a strong opinion, that these figures in specula being attained, the Gregorian reflecting telescope will take a high, if not the highest, place amongst optical instruments for power and distinctness, and should be glad to hear that its power had been proved.

ART. V.—*Extracts from Professor Airy's Translation of Encke's Dissertations on the next return of Pons' (Encke's) Comet in the year 1832.*

THE astronomers of England are under great obligations to Professor Airy for having translated this admirable dissertation, which appeared in the *Astronomische Nachrichten*, Nos. 210 and 211, and not less so to the Syndicate of the University Press at Cambridge, who have enabled him to distribute it gratuitously, by defraying the expence of printing it.

Professor Airy has added several theorems equivalent to those which Encke has employed in the theory of this comet, and a short statement of the reasoning on which he relies as proving the existence of some cause whose effects are the same as those of a resisting medium.

As this interesting pamphlet is too long and profound for a popular journal, we shall content ourselves with some extracts from it.

“Encke's comet,” says Professor Airy, “is undoubtedly one of the most remarkable bodies belonging to our system; and the conclusions which have been derived from its successive appearances are among the most important, with regard to the physics of the universe in general as well as to astronomical science in particular, which the present century has produced. The methods, by which the necessary calculations are made, have never been practically employed in this country, and are little known; even to those among us who are acquainted with the ordinary operations of physical astronomy. This Essay is, I believe, the first publication which contains a complete abstract of Encke's theory and its comparison with observation. If by circulating a translation I shall excite the curiosity of one reader to possess himself more completely of the theory and the facts of this singular body, I shall think my trouble well repaid.

“This comet,” continues Mr Airy, “was first seen by Mechain and Messier in 1786, but they observed it only twice, and were therefore unable to determine the elements of its orbit. Miss Herschel discovered it in 1795, and it was observed by several European astronomers. In 1805 Pons, Huth, and

After a comparison of the various observations in this remarkable body, Encke proceeds as follows:

Elements of Pons' Comet for 1832.

Longitude of perihelion $157^{\circ} \ 21' \ 2'',4$ } **Mean Equinox, 1892,**
 Ω **-** **334 \ 32 \ 5,2** } **May 4,**

Inclination of the orbit $13^{\circ} 22' 12",3$

Angle of eccentricity $57^{\circ} 48' 6",3$

Mean daily sidereal motion $1071",09598$.

1832. 0,3010. Mean Paris Time.	Comet's Right Ascension.	Comet's Declination.	Log. Dist. from Earth.	Log. Dist. from Sun.
Jan. 0,	343° 43',9	+ 1° 6',7	0,3545	0,3162
4,	344 35,1	1 23,5	0,3570	0,3173
8,	345 30,7	1 42,6	0,3590	0,2980
12,	346 30,8	2 4,0	0,3605	0,2883
16,	347 35,5	2 27,7	0,3614	0,2782
20,	348 44,5	2 53,7	0,3617	0,2677
24,	349 57,8	3 22,0	0,3615	0,2567
28,	351 15,5	3 52,6	0,3606	0,2451
Feb. 1,	352 37,5	4 25,4	0,3591	0,2330
5,	354 3,8	5 0,4	0,3569	0,2203
9,	355 34,5	5 37,6	0,3540	0,2069
13,	357 9,7	6 17,0	0,3504	0,1928
17,	358 49,9	6 58,6	0,3461	0,1779
21,	0 35,2	7 42,6	0,3411	0,1621
25,	2 25,8	8 28,9	0,3352	0,1453
29,	4 22,1	9 17,3	0,3284	0,1274
March 4,	6 24,9	10 7,9	0,3208	0,1083
8,	8 34,4	11 0,9	0,3123	0,0878
12,	10 51,6	11 56,1	0,3027	0,0656
16,	13 17,5	+12 53,6	0,2920	0,0416
20,	15 53,0	13 53,1	0,2802	0,0156
24,	18 39,1	14 54,6	0,2671	9,9872
28,	21 37,7	15 57,6	0,2526	9,9559
April 1,	24 50,6	17 1,8	0,2364	9,9213
5,	28 20,0	18 6,5	0,2184	9,8827
9,	32 7,9	19 10,1	0,1981	9,8396
13,	36 17,7	20 10,7	0,1750	9,7911
17,	40 50,9	21 5,2	0,1483	9,7369
21,	45 48,8	21 47,9	0,1170	9,6774
25,	51 4,9	22 10,0	0,0794	9,6162
29,	56 24,4	21 58,3	0,0335	9,5632
May 3,	61 9,5	20 58,0	9,9782	9,5365
7,	64 35,7	19 1,0	9,9151	9,5500

May 11,	66° 19,4	16° 10,4	9,8487	9,5962
15,	66 32,8	12 37,2	9,7831	9,6562
19,	65 36,7	8 24,8	9,7201	9,7169
23,	63 48,0	+ 3 33,7	9,6602	9,7731
27,	61 17,6	— 2 1,7	9,6034	9,8235
31,	58 7,8	8 28,3	9,5502	9,8684

“ In order to judge of the possibility of seeing the comet in our northern hemisphere, I have formed the following table, which shows the time of the comet's setting and the time of sunset for the latitude of Berlin, together with the days of 1828, on which the comet's distance from the sun was the same as on the days of 1832 in the first column. For all the circumstances of this comet seem to concur in showing that the distance from the sun, or, which amounts to the same, the intensity of the reflected light, determines its visibility. The quantity of light, or the apparent magnitude of the comet, has a very slight influence.

	Comet Sets.	Sun Sets.
1832. Jan. 0,	10 ^h 24'	3 ^h 54'
16,	9 44	4 14
Feb. 1,	9 11	4 43
17,	8 46	5 13
March 4,	8 31	5 43
20,	8 27	6 12
April 5,	8 39	6 40
21,	9 11	7 7
29,	9 23	7 23
May 7,	9 4	7 35
15,	8 1	7 47

“ From this it would seem that though the hope of seeing the comet is faint, yet it is not to be entirely given up. Struve's first efficient observation in 1828, was on the 26th of October. On the 17th of February 1832, the comet will be at the same distance from the sun as on that day, and will set 3½ hours after the sun. Its distance from the earth, however, will be four times greater than in 1828, a difference of position which greatly increases the difficulty. About the time of its greatest brightness it sets only two hours later than the sun. Yet as Struve observed it on December 26, 1828, though under unfavourable

circumstances, only 44^m before its setting, and not two hours after the sun's setting, it may perhaps be again possible to make an efficient observation in April. At least it is very much to be wished that Struve and those astronomers generally who can command instruments of great light, would take the trouble of convincing themselves by a strict examination of the presumed place of the comet, whether a trace of it can be discovered.

“ After its passage, May 4, until June, the comet comes nearer to the earth than it has done upon any former appearance. Though it does not arrive at the maximum of its possible brightness and of its apparent magnitude at the same time, yet it will be easily found even with the naked eye, if its place is but tolerably known. On May 7 it sets 1½ hour after the sun for a south latitude 34°. Unfortunately, through the return of Mr Rumker, we have lost the prospect of obtaining observations from Paramatta. The chasm, however, will be filled up by observations made at the Cape of Good Hope by Mr Fallows, to whom we already owe so many excellent series of determinations.

“ The review of the course which I have hitherto adopted in my calculations, from the first moment in which I was fortunate enough to discover the periodical return of the comet until the formation of this last system of elements, is rendered somewhat difficult by the dispersion of the essays, each of which contained the results which had been obtained at the time of publication, through different periodicals. It appears to me, therefore, not superfluous to subjoin a short statement comprising their principal results, which may answer the same purpose as this troublesome research.

“ The first calculations of perturbations applied to the periods 1805—1819 and 1795—1805, led to this surprising conclusion, which has since received further confirmation, that the magnitude of the semi-major axis of the orbit, cleared from perturbations and reduced to a given instant, is obtained smaller from the later revolutions than from the earlier. (Bode's *Astronom. Jahrb.* 1822, p. 200.) A somewhat more accurate repetition, in which also the return of 1786 was included, established this difference. In fact the pure elliptical time of

revolution of the comet, cleared from perturbations and reduced to the time of the perihelion passage 1805, appeared to be

from the period 1786—1795...	1208,112 Days...	3 Revolutions
1795—1805...	1207,879	—...3 —————
1805—1819...	1207,424	—...4 —————

(Bode's *Astronom. Jahrb.* 1823, p. 215.) The observations of 1786, 1795, 1805, 1819, were strictly examined, and their greater or less uncertainty might perhaps amount to 1 or 2 minutes, but not to so much that there could have been a possibility of errors as great as must have found place if a uniform period of revolution had been assumed. There remains, then, nothing else for the prediction of future appearances than the trying a new explanation which may correspond with the earlier observations. (Bode's *Astronom. Jahrb.* 1822, p. 183—196; 1826, p. 128—131.)

The most natural, and in fact almost the only explanation which this phenomenon admits of, appears to me, (an opinion in which Olbers concurs,) to be afforded by the hypothesis that the comet experiences a resistance in its course, which (as the existence of a perfect vacuum is improbable) may be exercised by the medium extending through all space. Its small density or other circumstances may be the reason that the denser planet-masses are not affected in the same manner. (Bode's *Astronom. Jahrb.* 1826, p. 133.) It is the simplest, since it is evident that the epochs of the perihelion may be connected by a formula including a term depending on the square of the time. Thus the epochs which serve for basis to the numbers above are very nearly represented by

$$1207,6564 n - 0,0542 n^2$$

where n represents the number of revolutions since 1805. It is also almost the only one, because any other, founded on the difference of the comet's constitution from that of the planets, might perhaps give a period of revolution that would not harmonize with our sun's mass, or Gauss's constant k , but would never give a continued variation of periodic time following a regular law (Argelander, *comet of 1811.*) The conviction of

its necessity was in the first years so strong that it was used for the prediction of the appearances of 1822. (Bode's *Astronom. Jahrb.* 1823, p. 217.)

"The event confirmed this hypothesis. The discovery of the comet in the year 1822 by Mr Rumker proved that the mean error of the ephemeris thus computed was only 5'. (*Astr. Nachr.* vol. ii. p. 38.) Hence the urgency of using for the ground of the hypothesis more accurate constants than the mere assumption of a diminution in the time of its revolution. On the whole, it was here of little importance whether the peculiar assumptions corresponded strictly with nature. As the comet is almost always observed at the same part or nearly so of its orbit, and consequently of space, every assumption will sufficiently represent the observations, which was the principal object to be attained; it being premised that the periods of revolution are thereby regularly successively lessened."

After explaining his hypothesis showing its agreement with observation, Encke concludes thus:

"If I may be permitted to express my opinion on a subject which for twelve years has incessantly occupied me, in treating which I have avoided no method, however circuitous, no kind of verification, in order to reach the truth as near as lay in my power; I cannot consider it otherwise than completely established that an extraordinary correction is necessary for Pons' comet, and equally certain that the principal part of it consists in an increase of the mean motion proportionate to the time. Another question, which is properly more physical than astronomical (as in strictness the determination of future appearances from past observations is the chief object of astronomy) is this; whether the hypothesis of a resisting medium gives the true or probable explanation; though hitherto no other appears to me to have equal weight."

Professor Airy concludes his appendix with the following observations.

"Now Encke has stated that his hypothesis, which represents all the later observations within a few seconds, does also represent the earlier observations within about eight minutes: and a part of this he thinks is due to the inaccurate calculations of perturbation. Consequently the supposition of no re-

sistance must be enormously in error for some of the Appearances: and there can therefore scarcely be a doubt that the hypothesis of a resisting medium, or something which produces almost exactly the same effects, is the true one.

“It will be observed that these conclusions depend entirely on calculations made by Encke, and which have not (I believe) been repeated by any other person. As far, however, as the skill and experience of one calculator can remove all doubts upon the accuracy of the results, they may be considered as perfectly certain. And I cannot but express my belief that the principal point of the theory, namely, an effect exactly similar to that which a resisting medium would produce, is perfectly established by the reasoning in Encke's memoir.

“For the convenience of those who may wish to construct the orbit of this comet, I subjoin the values of the elements which Encke has omitted.

Perihelion distance = 0,3435	} The earth's mean distance
Aphelion distance = 4,101	
Semiminor axis of orbit = 1,187	
Periodic time = 1210 days.	

The other elements will be found in page 240. The place of perihelion, it will be seen, coincides nearly with the descending node. All these elements are considerably altered by perturbation.”

ART. VI.—*Account of Experiments on the Performance of some Steam Engines in Cornwall.* By W. J. HENWOOD, F. G. S. &c. (Communicated by the Author.)

SIR,

A TRIAL of the performance of some Steam Engines, being part of a survey of the Mines of this county, on which I am now engaged, and being through the kindness of R. W. Fox, Esq: favoured with the use of an *Indicator*, (an instrument for determining the amount of the expansion of steam in the cylinder) at the desire of several of my friends, men of science, and managers of mines, I have made some experiments; and I beg the favour of a place in your pages for a detail of

them. It does not seem requisite to mention the reasons which have led to the selection I have made.

I commenced an experiment on Wilson's Engine at Wheal Towan on the 22d of November last, at 2h. 32m. P M., and finished at 3h. 5m. P M. on the following day. This engine has a cylinder of 80 inches in diameter and makes therein a stroke of 10 feet, and of 8 feet in the shaft. The

	Fms.	ft.	ins.	Diameter inches.	Weight in lbs.
House lift is,	44	1	9	13	15309 3 (Note B.)
Tye -	43	5	9	15 $\frac{7}{8}$	22658
Rose -	32	5	9	16 $\frac{1}{8}$	17527 1
Crown -	18	5	8	16 $\frac{1}{8}$	10074 5
Puppy -	9	4	2 (Note A.)	12 $\frac{1}{2}$	3097 5

68666 4

The quantity of coal consumed was 50 bushels, and its weight 5003 lbs. (see Note C :) a portion of this on being dried until it refused to yield more moisture, lost 1-16th of its weight. The strokes made by the engine during the trial were 7881.

The oil used in the engine was - 1 pint.

Grease in ditto - - - 17 lbs.

Ditto in shaft - - - 3

The mean temp. of the hot well was about 95°

Ditto - injection water 63° (See Note D.)

Ditto - engine room 76

Ditto - boiler shed 76

Ditto - external air 53

The pressure of steam in the boiler varied from about 25 to 40 lbs. on the square inch, being on a mean about 35 (see Note E.) On all these points I made horary observations (see Note F.) From the foregoing data the performance will be found to have averaged 86585079 lbs. lifted one foot high by the consumption of one (measured) bushel of coal. Mr Grose is the engineer at this mine.

My trial of Hudson's engine at East Crennis was begun at 9h. 28m. A. M. of the 30th November, and terminated at 7h. 55m. A. M. of the 1st of December. The cylinder of this engine is 76 inches in diameter, the stroke in it 10 ft. 3 in., and in the pump 7 ft. 2 in. The lifts are

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	Fms. ft. ins.	Diameter inches.	Weight in lbs.
House	6 3 2	13	2256 4
Tye	26 3 3	18	17588 6
Rose	44 5 8	18	29783 8
Crown	33 0 7	17	19563 5
Puppy	12 1 3	14	4893 8

74086 1

34 bushels of coal were consumed, of which the weight was 3005 lbs. A portion thereof being dried in a similar manner to that before described, lost 1.21 of its weight. The strokes made were 4717.

Oil used in the engine - - 1 pint.

Grease in ditto - - 12 lbs.

Ditto in the shaft - - 5

The mean temperature of the hot well was about 87. (See Note G.)

Ditto - - injection water 67

Ditto - - engine room 60

Ditto - - boiler shed 66

Ditto - - external air 45

The steam in the boiler varied in pressure from about 14 to 25lbs. on the square inch, the mean being perhaps near 18lbs. The preceding details show the average duty during this experiment to have been 73654606 lbs. lifted one foot high by the combustion of one (measured) bushel of coal. Of this mine Messrs Sims and Son are the engineers.

At Binner Downs (Messrs Gregor and Thomas, engineers,) I commenced a trial of Swan's engine at 10h. 59m. A. M. of the 8th December, which was finished at 0h. 2m. P. M. of the 9th. This engine has a cylinder of 70 inches in diameter, a stroke therein of 10 feet, and in the shaft of $7\frac{1}{2}$ feet. The lifts are

	Fms. ft. ins.	Diameter inches.	Weight in lbs.
House	3 3 3	10	724 3 (Note H.)
Tye	40 2 8	$18\frac{1}{8}$	27175 2 (Note I.)
Rose	41 2 0	$17\frac{1}{8}$	24792 5
			51967 7

The coal consumed was 60 bushels, which weighed 5561 lbs. : a portion being dried, as before, lost 1-10 of its weight. The number of strokes made was 11258.

The Oil used in the engine was - 1 pint.

Grease in ditto - - 12½ lbs.

Ditto in the shaft - - 3

The mean temperature of the hot well was about 88°

Ditto - - injection water 52

Ditto - - engine room 66

Ditto - - boiler shed 75

Ditto - - external air 53

The pressure of steam in the boiler varied from 35 to 50 lbs. on the inch, averaging about 45 lbs. These data show the duty during this trial to have been on a mean 73877810 lbs. lifted one foot high by the use of each measured bushel of coal. In measuring the length of the lifts, I have followed the mode agreed on at Wheal Towan between Mr Lewin, Sir John Rennie's assistant, and myself: viz. taking from the top of the *blast holes*, in the *windbore*, to one-half the height the water rises in the *collar launder*, above the pump head. At Wheal Towan there are facilities for approximating by the use of a float, the actual quantity of water, delivered at the pump head; and of these I have availed myself, although more pressing engagements have not permitted my yet calculating the results. During the trial made by Sir John Rennie and myself; however, the observed and calculated quantities were as 85 to 92 or thereabout.

From the want of coincidence observable in the weight of the bushel of coal at the mine before named, it is obvious that the measure thereof is not to be relied on as a standard of power applied to the steam engine. But having weighed the whole, and dried a portion of each, we find the actual quantity of *dry coal* used in each of these trials; of which the results will be nearly represented by the following numbers. (Note K.)

Engine.	Duty per bush. of coal as measured.	Weight of bush of coal damp. lbs.	Weight of bush. of coal dry. lbs.	Duty per 84 lbs. of damp coal. lbs.
Wheal Towan	86585079	100.06	93.80625	72687853
East Crennis	73954606	88.3823	84.1736	70003555
Binner Downs	73877810	92.683	83.415	66956572

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Engine.	Duty per 84 lbs. of dry coal. lbs.	Duty per one lb. of damp coal. lbs.	Duty per one lb. of dry coal. lbs.
Wheal Towan	77533710	865331	923020
East Crennis	73502699	833363	875032
Binner Downs	74395923	797102	885665

The first consideration in importance, although the last in place, is the actual weight lifted by each engine with a given cost. In this calculation I include only the value of the *materials* employed. I believe no one in either of these experiments *remained* in the shaft; the only supervision of the pumps, &c. being confined to visits from the workmen in their descent to, and ascent from, the lowest level, in which they were employed.

The average prices of materials at the before-mentioned mines, are

Coals	41s. 0d. for 72 measured bushels.
Grease	45 6 per 112 lbs.
Oil	- 4 2 per gallon.

These give for Wheal Towan about 1085 Tons.

East Crennis - - 870 (Note L.)

Binner Downs - 1006

lifted one foot high, for the expence of *one farthing*.

I remain, &c.

W. J. HENWOOD.

(Note A.)—Since I assisted Sir John Rennie at a trial of this engine, these lifts have been altered in diameter: the others have undergone no change.

(Note B.)—The temperature of the water in the pumps varies from 65° to 73°, but I have made no correction for it, nor for the impurities contained in the liquid: this is equally applicable to the other engines herein mentioned.

(Note C.)—My friend Captain Vivian observes to me, that this coal is "*very slaty*," which he considers to increase the weight, whilst it diminishes the efficiency thereof.

(Note D.)—The water enters the cistern almost immediately as it is pumped out of the mine; this is also the case at East Crennis, but not at Binner Downs.

(Note E.)—As I write somewhat pressed for time, I beg in all my remarks on the pressure of the steam, to be understood as speaking less confidently than I do on all the other points.

(Note F.)—This remark also applies to all the observations on these particulars, on the other engines.

(Note G.)—The water in the lifts varied in temperature from 63° to 73° . See foregoing note.

(Note H.)—The stroke in this pump is $5\frac{1}{2}$ feet, in the others $7\frac{1}{2}$ feet.

(Note I.)—Temperature from $72\frac{1}{2}^{\circ}$ to 74° . See preceding note.

(Note K.)—This proceeds on the assumption that the moisture contained in coal is passive during the combustion of the latter. This is *believed* to be untrue, but of its true value we shall probably long remain ignorant. The variable quantity of this moisture, of course depends on the thickness of the heap of coal, and on the quantity of rain to which it is exposed.

(Note L.)—I believe this engine might have made very many more strokes in the same time, without a greater quantity of grease and oil. From which it would appear that the expence of these articles depends mostly on the size of the machine, and on the time, and is not in proportion to its velocity.

PERRAN-WHARF, NEAR TRURO, Jan. 2, 1832.

ART. VII.—*Observations on the Cause of Halos, and of the phenomena of Diverging and Converging Beams.* In a Letter from L. A. NECKER, Esq. Professor of Mineralogy, Geneva, to J. D. FORBES, Esq. (Communicated by Mr Forbes.)

MY DEAR SIR,

IN reading Dr Brewster's excellent Treatise on Optics in Lardner's *Cyclopædia*, in which so much new and valuable information is included in so small a space, when arrived at the chapter 33d of the 3d part, which comprehends halos, parhelia, and diverging and converging sunbeams, I was struck by some objections which occurred to me against the explanations offered by this celebrated philosopher, of these, and especially the latter, phenomena. If you think these objections, and the observations on which they are founded, worthy of Dr Brewster's examination, I beg you would mention them to him, with, however, all the deference which is due from a

mere student in this science, such as I am, to so renowned a master of it. If not, I trust you will be good enough, for my instruction, to give me your answer to my objections, and your explanation of the facts I am to mention.

FIRST, it is said, page 270, that "in cold weather, when particles of ice are floating in the higher regions, the two luminaries are frequently surrounded with the most complicated phenomena," &c. Moreover, all the 163d paragraph, from page 274 to page 277, is a comment on the assumption that such parhelia and large halos owe their existence to crystals of ice and snow floating in the air, and on the supposition that such phenomena belong to the northern regions of the globe, and occur in cold weather. Now it happens that I witnessed with some of my friends at Geneva, or rather Cologny, a village distant two miles east of this town, a very striking and beautiful parhelia, with two mock suns, two concentric circles, and a segment of an inverted circle, as H R C in Fig. 136, resting on the summit of the outer circle; in the third week of July 1880, (I have not at present the date of the day, but have preserved it at home together with a description and a drawing of the phenomena, which could be very easily sent to Dr Brewster if thought interesting,) between the hours of five and eight P. M. Here is then an instance of a parhelion occurring in a southern climate in the hottest season of the year, and in almost the hottest hours of the day. And what is remarkable, the weather had been before, was at the time, and continued to be for some days after, more than commonly hot over all this and the neighbouring parts of the continent. Nor was any hail storm heard of before, during, or after the phenomenon. So that if the air was charged with icy or snowy particles, it could be only at a height superior at least to 5000 metres above the level of the sea; for at the time when I was observing the phenomenon, I also observed that Mont Blanc and all the parts around and above it were quite clear.

SECONDLY, I venture to object to Dr Brewster's explanation of what he calls converging and diverging beams of the sun, —phenomena which I had occasion to study and analyze; 1st, That there is really nothing existing as what is in common acceptance, as well as in poetical language, called *sun's beams*;

2d, That in the phenomena alluded to, and figured by Dr Brewster in figures 138 and 139, and explained p. 277—279, there exists nothing converging nor diverging, and so I concur entirely with him in asserting that the phenomenon of divergence and convergence is entirely one of perspective. But for that same reason it is impossible for me to admit his explanation p. 279, exemplified by the supposition of meridians; for meridians are in four lines diverging from one pole and then converging towards the opposite pole.—Now for the developement of these objections. I will first observe that it is only necessary to look at the sun in a clear and quite transparent sky either with your own eyes, if they be strong enough to bear this light, or through a blackened glass, or by the reflexion of still water, to see that the sun has no more beams than he has eyes, nose and mouth, although painters have always been fond to adorn with those imaginary features the apparently too uniform figure of this luminary. *Secondly*, That if by *beams* you understand the *optical rays* proceeding from the sun, which is a quite different thing from what is generally meant by *beams*, such rays, owing to the immense distance of the sun from which they proceed, and to the small extent of the earth's surface on which they fall, when compared to that distance, are generally reputed to arrive parallel on any given region of the earth, so that they can never be supposed to have that radiating appearance around the sun's orb as is represented, Fig. 138. Here is then what I conceive to be the real cause of that appearance, and the circumstances required for its taking place. The two most essential of these requisites are a hazy atmosphere and a quantity of small disjointed clouds floating in the sky, sometimes apparently collected near one another, but leaving always larger or smaller interstices between them. Now, whenever such clusters of clouds will be interposed between the spectator and the sun, these clouds *c*, being at a distance of several leagues from the spectator, will intercept each of them a pencil of the sun's rays, DD'D' Fig. 4, Plate II., equal in thickness to the surface of the section of each cloud *c*, supposed to be made perpendicularly to the direction of the rays; and as these rays are all parallel to each other, it follows that the shadow produced by such an interception will extend indefinitely always with an equal breadth,

at the same time that the sun's rays moving freely and in a parallel manner through the interstices LL' of the clouds, will everywhere in their course fill exactly the very same space, equal in breadth to that of each interstice. So that in reality the space situated behind the clouds or opposite to the sun is as it were striped by more or less haze, but always parallel stripes or bands alternately dark DD' and luminous LL' , as is represented Fig. 1, where the plane of the drawing is supposed parallel to the direction of the rays, or having its common intersection with the horizontal plane directed east and west. Now it is necessary that the air be hazy or thick, that this difference of light and shadow in those empty spaces behind the clouds should be perceptible. In such circumstances, let us now suppose a spectator s placed on the surface of the earth FGA and near A , looking towards S or the sun, he will see a plane perpendicular to that represented in Fig. 1, or directed north and south, in which the clouds c , the dark stripes $DD'D''$, and the luminous bands $LL'L''$ will also be visible, but projected quite differently as they are in the figure. Owing to the perspective, the clouds c , some leagues distant, will appear very low above the horizon, while the parts situated near B , being very near, will be seen very high near the zenith; the parts of the bands or stripes $DD'D''D'''$ and $LL'L''$, situated far off between G and H , will appear very thin, while the parts of the same bands situated nearer A and B will assume a great extent as being much nearer the spectator, so that $DL'D'L'$ will appear as diverging upwards from the sun S , while $L''D'''$, &c. will appear diverging downwards from the same central point. In such a phenomenon, the clouds c and their projecting shadows DD' , &c. are the real and effective causes of these whole appearances, while the luminous interstices and the rays which pass through them LL' , &c. are in reality nothing, being an unchanged part of what exists always, and would take place even were not the clouds c present. Notwithstanding that, whether it be owing to the greater brilliancy of the luminous parts, or to our old prejudice about the beams of the sun, we generally pay attention only to the luminous bands, and believe them to be the effective phenomenon, while it is the dark stripes which are really the phenomena produ-

cing such appearances, and which, illuminating the unchanged spaces $L L'$, &c. give them their appearance of beams.

We come now to the so-called converging beams, which, according to Dr Brewster's explanation, p. 279, ought to be a natural consequence of the diverging beams, though he remarks himself justly, that they are in fact of much rarer occurrence. I had a very good opportunity of observing that phenomenon in September last (1831) from Cologny near Geneva; and there I saw that the same circumstances were required for its production as for that of the diverging beams, viz. a thick or hazy atmosphere, and small disjointed clouds floating in the sky; with this difference, however, that while in the case of the diverging beams the clouds and the hazy atmosphere must be on the same side with the sun, in the case of the converging beams they must both be on the opposite side. For while in the first case the clouds must be interposed between the spectator and the sun, in the second, the spectator must be placed between the clouds and the sun. This circumstance only excepted, every thing remains the same as in the preceding explanation but only reversed, and the same Fig. 1. will help us to understand what happens. Let the spectator, instead of being in s near A, be in s' near F, turning his back to the sun S and looking towards the clouds c and towards A B. Now the parts of the bands $D L D' L'$ near A and B being the farthest, will appear the thinnest, and those near H G and near the clouds c being the nearest, will appear broadest; at the same time that the parts near H will appear high towards the zenith, and the parts near B low towards the horizon, so that also a diverging appearance will be manifested best from a point opposed to the sun.

In the instance just alluded to of my observations in September 1831, the point towards which the beams appeared to converge, or from which they appeared to diverge, was a point particularly remarkable, and which probably attracted my attention to the phenomenon itself. It was the summit of Mont Blanc, which at that time appeared the central point of the beams, as will be seen in Fig. 5. As the hour of observation was between five and six in the afternoon, the sun in the be-

ginning of September being a little to the north of west, its opposite point was a little to the south of east, which is exactly the bearing of Mont Blanc from Cologne. The luminous parts or beams had that reddish hue which is generally observed in the eastern part of the sky when the atmosphere is hazy, and when the clouds near the sun on the west are of a golden colour, as they were at that moment. These eight or nine broad red luminous beams starting as it were from the top of Mont Blanc, while on the opposite side the sun was exhibiting above the Tara and the lake a splendid specimen of the diverging beams, engrossed at first all my attention, and I was much embarrassed to find the cause of those two simultaneous and analogous appearances in the east and in the west, and the more so that no continuity appeared between the luminous eastern and western beams, for the zenithal parts for a great space were quite clear and of a fine blue colour; when I happened to observe that each of the hitherto unnoticed slender dark beams, which limited and separated the luminous ones, had its origin at a small cloud suspended in the air. This gave me the key of the whole appearance, and what had before appeared so puzzling became quite clear.

It will now be easily understood how, in some circumstances, the phenomena of converging and of diverging beams may be entirely independent of each other; for the constitution of the sky is often very different in the eastern and in the western quarter. While it may sometimes happen, as in the case alluded to, that the same atmospheric constitution being at the same time extended over a large region, the haziness of the air, and the floating clouds required to produce both phenomena, may exist before as well as behind the spectator, and so allow him to witness in the same instant those very striking appearances.—I am, &c.

L. A. NECKER.

EDINBURGH, *Feb. 13th 1832.*

Observations on the preceding Letter. By the EDITOR.

THE remarks made in the preceding letter upon some parts of

my *Treatise on Optics* published in Dr Lardner's *Cyclopædia*, relate to two points.

1. To the explanation of the phenomena of haloes and parhelia, by the supposition that minute crystals of ice are floating in the atmosphere, and

2. To the cause of the phenomena of diverging and converging beams.

1. Of the various hypotheses which have been framed to account for natural phenomena, there are few which have a better claim to reception than that which refers the luminous circles which surround the sun and moon to the refraction of crystals of ice disseminated in the atmosphere. That such crystals do exist in the atmosphere is not a hypothesis but a matter of fact, and that they have such a crystalline form, and such angles, as will refract the rays of the sun and moon so as to form round them luminous circles of the very same diameter as those which are observed, is equally certain. The only thing necessary to convert the hypothesis into an undoubted fact, is to prove that the existence of particles of ice and of haloes, &c. are concomitant phenomena.

We do not pretend to be able to give a demonstration of this position, but we can adduce some facts which render it highly probable, and we may be able to remove the difficulty which has presented itself to Professor Necker.

1. In the arctic regions, where the existence of slender crystals of ice is proved by the sense of touch,—by their pricking the skin like needles,—and raising blisters on the face and hands, haloes and parhelia are constantly seen at the same period of the year. In summer, when this *frost-smoke*, as it is called, does not appear, mock suns are seldom visible. See Art. GREENLAND, by Sir Charles Giesecké, *Edin. Encyclopædia*, vol. x. p. 487.

2. In the temperate regions haloes and parhelia *almost always* occur in the winter, spring, and autumn months, when, from the coldness of the air, it is most likely that crystals of ice exist in the upper regions.

3. Haloes of 47° and 94°, (the only ones which crystals of ice are supposed to produce,) seldom, if ever, occur in the tor-

rid zone where crystals of ice are less likely to occur in the upper air than in more temperate regions.

Professor Necker founds his objections to the hypothesis on an observation of a parhelion which he himself made at Cologny near Geneva in the *third week of July 1830*, between 5^h and 8^h P. M. As this occurred during a very warm season, he infers from it that there could have been no icy particles at a lower level than 5000 metres above the sea, for Mont Blanc and all the parts around and above it were quite clear.

This observation is certainly a very interesting one, but it does not present us with any evidence capable of proving that there were no icy particles in the upper regions of the air at Cologny. The clearness of Mont Blanc seems to us of no consequence whatever; for we know that there are local depressions of temperature in the atmosphere, which are often marked by the existence and passage of a hail storm, occupying an extremely limited breadth, and producing no perceptible change in the temperature of places not far removed from it. There can therefore be no difficulty in supposing, that, in some part of the atmosphere between Professor Necker's position at Cologny and the sun, there was a depression of temperature capable of producing crystals of ice. Whether this cold occurred at a greater or a less height than 5000 metres, is of no consequence in the present argument.

I shall only add, that in the warmest year which I recollect in Scotland, I saw snow fall in the middle of July in flakes *two inches and a half* long; and I believe it is a fact, that in warm climates hail often falls at the hottest hour of the day, and at the warmest season of the year, without indicating its approach by a previous change of temperature. When minute portions of water are precipitated in the upper atmosphere in summer, they may be converted into such slender spiculæ of ice, that they might disappear by heat and evaporation before they reached the ground, having shown their existence only in the optical phenomena which they produce round the sun and moon.

2. We now come to the subject of Converging and Diverging Beams. Dr Robert Smith of Cambridge, (*Optics*, vol. ii.

Rem. i.) was, so far as I know, the first person who applied these names to the phenomena in question, and who gave a correct explanation of their cause. Professor Necker criticises the use of the word sunbeams, and remarks, "that the sun has no more beams than he has eyes, nose, and mouth." It is very true that the "sun *has* no beams;" and I believe that neither Dr Smith nor I have said that he has such appendages; but although the sun *has* no beams, there are nevertheless such things as sunbeams, or portions of the sun's light bounded by shadow, which are rendered visible by the reflexion of his rays from particles of dust floating in a room, or from vapours or exhalations in the atmosphere. The word sunbeam is a term in our language universally understood, and having a perfectly definite meaning; and its application is so appropriate to the phenomena of which we are treating, that no English writer either would or could use another.

Dr Smith's explanation of diverging and converging beams is so satisfactory, that, if there had been room, I would have given his large diagram, and entered into a full explanation of the phenomena. This was, however, quite inadmissible in a Treatise on Optics limited to a small duodecimo volume; and I was therefore obliged to compress the explanation into a single paragraph, in which I have no doubt illustrated the truth of the poet's observation, *Brevis esse laboro, obscurus fio*. But, brief and obscure as that explanation may be, it is perfectly correct, and identical with that of Dr Smith and Professor Necker; and, by comparing the beams of light to planes passing through the meridians of a globe, I have been often able to convey to different persons a satisfactory idea of the phenomenon, and of its cause.

If Professor Necker will look again to the paragraph in p. 279, which he criticises, he will see that it is entirely an explanation of the *converging beams*. The cause of the apparent divergency of the sun's light in passing through interstices among the clouds is so obvious, that I never thought of explaining the cause of it. In order to explain their *convergency*, I begin by supposing the diverging beams to be fifteen in number, and inclined 24° to one another, like the meridian of a

globe, and *I suppose planes to pass through all these meridians*, (a supposition which Professor Necker has overlooked.) I then suppose the axis or common intersection of all these planes to pass through the eye of the observer and the sun ; and I assert, that the observer will see the fifteen beams converging to a point opposite to the sun. This illustration of the convergency of the beams to a point below the horizon is perfectly correct in principle, and, in my opinion, not only a popular one, but the only one that I can find that is suited to the limited space which was destined to receive it.

Professor Necker is of opinion that converging beams cannot be seen unless there are small disjointed clouds, as well as a hazy atmosphere, between the observer and the sky opposite to the sun. It appears to us, on the contrary, that it is not necessary that the clouds be interposed between the observer and the part of the sky where the converging beams are seen. The beams may be produced by the clouds between the observer and the sun ; and, if they are not obstructed in their passage to the opposite horizon, their convergency may be seen without the aid of any other clouds. Hence there may be *three* cases of converging beams,—the *first* where the beams are formed by the clouds between the observer and the sun, and where there are no clouds in the opposite half of the sky ; the *second* where there are no clouds between the observer and the sun, but where the beams are separated by clouds in the opposite half of the sky ; and the *third* where the whole sky is cloudy, and where the beams separated by the clouds near the sun are again modified by passing through the openings of clouds in the opposite part of the sky. This last case is the most common, and the only one which has been observed.—See this *Journal*, Old Series, No. iii. p. 136.

ALLERLY, *February 20th 1832.*

ART. VIII.—*On the Horary Oscillations of the Barometer near Edinburgh, deduced from 4410 Observations; with an Inquiry into the Law of Geographical Distribution of the Phenomenon.* By JAMES D. FORBES, Esq. F. R. S. E. F. G. S. Honorary Member of the Yorkshire Philosophical Society, &c. Abridged from the *Trans. of the Royal Society of Edinburgh*. (Read 4th April 1831.)

THE science of Meteorology must be ranked, at the present moment, among the most rising branches of natural knowledge. The transition from the hasty generalization which always marks the embryo state of science, to the application of sober inductive analysis, is one so important, and so truly interesting, as to repay amply the philosophical abstinence which it imposes. No more important lesson, indeed, can be learned, than from the very examples of crude speculation, which, for centuries, the progress of this subject has afforded among the multitudes whose scientific acquirements are limited to the art of consulting a weather-glass, or registering a thermometer, little imagining that the very science they affect to cultivate, ranks among its phenomena the interwoven effects of remote and recondite causes,—a science which, to use the words of Mr Herschel, is “one of the most complicated and difficult, but, at the same time, interesting subjects of physical research: one, however, which has of late begun to be studied with a diligence which promises the speedy disclosure of relations and laws, of which, at present, we can form but a very imperfect notion.”

One of the most important features of recent improvement in the mode of studying the group of facts which meteorology presents to us, is the analytical method of discussing observations, extracting, as it were, from the data which nature presents to us, laws which shall best represent them, instead of framing clumsy hypotheses, to which it became a secondary object to apply the facts. No better example of the method can be given than Humboldt's masterly essay on *Isothermal Lines*; and the same distinguished traveller was the first who threw into anything like lucid order, the class of phenomena which it is the object of this paper to discuss,—the Horary Oscillations of the Barometer.

These atmospheric tides were first discovered in a decisive manner at Surinam, in 1722, by a Dutch philosopher, whose name has not descended to us ; and the history of their subsequent developement is too well known to require even an abstract in this place. It has been reserved for the nineteenth century to ascertain almost every thing connected with them besides their bare existence, and even that was not established in the temperate zone previous to the observations of Ramond, little more than twenty years since. But it was reserved for Humboldt, after much practical labour in the first years of the century, to draw up the admirable analysis of the subject which appeared in the third volume of the "*Relation Historique*," published in 1825. From that time a new impulse was given to the labours of observers ; and during the last five years many important registers have thrown light upon the modification which the barometric oscillation undergoes in different latitudes, and at various heights above the sea. The subject had always appeared to me one of singular interest, and accidental circumstances first induced me to prosecute it experimentally, during some months' stay at Rome in the spring of 1827. The period was very short, but as I frequently made from twelve to fifteen observations in a day, I was enabled, very satisfactorily, to trace the variation from hour to hour, and to ascertain the periods of maximum and minimum, though too brief to determine the amount accurately, which came out, in all probability considerably too small.* On my return to Scotland in autumn of the same year, I resolved to institute immediately a series of observations in latitude 56° , a more northerly station than any at which observations have, as far as I know, yet been made for this purpose, excepting those of Captain Sir Edward Parry. These I have pursued almost without intermission, up to the close of 1830 ; and when it is recollected how rare observations, sufficiently long continued, have been in the higher latitudes, and especially how trivial have been the donations of Great Britain to this branch of meteorology, I hope that, with all the defects inseparable from

* A full account of these observations was published in the *Edinburgh Journal of Science* for January and April 1828.

the efforts of a solitary observer, the results of this inquiry may not be unacceptable.

These observations, amounting in all to 4410 in number; comprised between the years 1827 and 1830, were made at Colinton House, four miles south-west of Edinburgh, in latitude $55^{\circ} 55' 20''$ N. and longitude $12^{\text{m}} 57^{\text{s}}.5$ west of Greenwich, deduced trigonometrically from the Calton Hill Observatory, and at 410.5 feet above the mean level of the sea, determined by accurate levelling. Four months observations, however, at the commencement of 1828, were made in Edinburgh, but reduced to the same level of 410.5 feet, in order to prevent their affecting the mean absolute height. Five observations were made daily of the barometer and attached thermometer, from 8 to $8\frac{1}{2}$ A. M., at 10 A. M. about 4 P. M., and at 8 and 10 P. M.; in order to detect the morning and evening maximum, and afternoon minimum. The same instrument has not been used throughout these observations. For the greater part of the last two years, however, I have used a barometer in which I put great confidence, the mercury having been boiled in the tube by myself with every precaution, and with every part of which I am thoroughly acquainted. It is of the mountain construction, has an adjustable level, and attached thermometer, and requires no correction for capillarity. Though the other barometers were not so unexceptionable, they were both furnished with attached thermometers, and one was on the mountain construction; indeed the nature of the deductions which I have to make being entirely confined to *differences*, without regard to absolute height, the nicety of the instrument is of little importance, considering the great number of observations required to be combined, in order to obtain any trustworthy result. Any constant cause, rendering the height of the mercurial column at one hour of the day different from that at another, independent of the atmospheric tide, must be sedulously guarded against; but there is nothing worth mentioning which can produce this effect, except change of temperature. The attached thermometer must therefore be considered a *sine qua non*. The corrections on this account were applied to the monthly means; and the table I have employ-

ed is that of Professor Schumacher : it may safely be affirmed, that, with rare exceptions, the tables commonly employed in this country are more or less erroneous, and some very strikingly so. The whole of the observations are thus reduced to 32° Fahr. I may also add, what is by no means unimportant, that with hardly any exceptions (amounting to not above a few dozens in all,) the whole of these 4410 observations were made personally by myself.

In the numerical reduction of the observations, I experienced a very harassing difficulty. The omissions at particular hours, which could not fail to occur occasionally, in the attempt of an individual to register the barometer five times a-day for so long a period, where they happened during extreme states of pressure, would, it is easy to see, materially affect the monthly means ; and although the reduction was continued in the hope that the combination of a sufficient number of months would destroy the irregularity, it became obvious, that any attempt to draw more delicate conclusions, such as the influence of the seasons, would be vain ; and, finally, I was led to distrust the accuracy of even the general result. Under this impression, I resolved to repeat the whole reductions upon a plan which I had previously employed with success, and which has fully answered my expectations. As the great object was to avoid the inequality of absolute height, which might vary indefinitely on different days, and preserve only the differences corresponding to different hours, I selected one time of day at which the barometer was most regularly observed, and, under the columns corresponding to the other hours, inserted the differences as they were + or — of the barometer and attached thermometer, from the heights at the standard hour inserted in the first column, and the means for each month of the principal column and the subsidiary ones being taken, the corrections (total or partial respectively) for temperature were applied, from which mean results a new set of absolute heights might be deduced. By far the greater deviations from the mean, on the account above alluded to, were thus avoided, and though particular omissions no doubt affected the partial means, the errors were greatly smaller in amount.

The general results of the monthly means fully justified my

expectations. The extent of the deviations on both sides, from the mean oscillation given by the monthly results, was greatly diminished, and likewise the number of times which the oscillation came out *negative*, or in the wrong direction.

The following are the general means,* the spring period being understood to include March, April, May; summer, June, July, August: autumn, September, October, November; and winter, December, January, February.

SEASONS.	No. of Obs.	8 A. M.	10 A. M.	4 P. M.	8 P. M.	10 P. M.
Spring,	1159	29.3651	29.3589	29.3438	29.3618	29.3640
Summer,	977	29.3896	29.3820	29.3715	29.3812	29.3866
Autumn,	1107	29.4378	29.4422	29.4286	29.4360	29.4312
Winter,	1167	29.4386	29.4447	29.4416	29.4447	29.4425
Sum,	4410	117.6311	117.6278	117.5855	117.6237	117.6248
Mean.		29.4078	29.4070	29.3964	29.4059	29.4061

It thence appears that the principal oscillation from the first morning observation to the afternoon one, amounts to—.0114 inch; that from the afternoon to the second evening one, to +.0097: the ratio is 1 : 1.18, which is considerably less than the best observations give for the south of Europe. In the north we have no data of comparison, since, as far as I know, no register capable of affording satisfactory results for the evening maximum has *ever* been kept in any part of Britain.† In the comparisons I am about to institute with the observations made in other parts of the world, I shall adopt the oscillation between 10 and 4, or—.0106 inch, since this agrees nearly with the usual hours of observation, as it has rarely happened that meteorologists have taken the trouble to observe the barometer at two periods near one another. The amount just named exceeds somewhat the oscillation observed by Sommer at Königsberg, in latitude 54° 42', which has hitherto been referred to as the solitary result from northern Europe.‡ We shall afterwards see that the amount I have assigned, agrees much more close-

* The extended tables containing the monthly results, will be found in the original Memoir in the *Edin. Trans.*

† I do not except Mr Daniell's Observations for reasons assigned below.

‡ Humboldt, *Relation Historique*, 4to edition, tom. iii. p. 306.

ly with the strict analogy of other observations in more southern latitudes. Besides, the hour at which the afternoon minimum was observed by Sommer was much too early in all probability, being part of the time at 2 P. M., and the remainder at 3 P. M. The hour in temperate climates is probably later than is generally assigned: nor does it add much to our confidence in the results, that Bessel informs us, that "notwithstanding the minuteness of the amount, it is recognized in each annual mean*," when in my tables it is eliminated in 28 out of 38 monthly means, notwithstanding its extreme minuteness in winter. In London, where the amount is more than twice as great as here, and the observations are made only twice a-day, but with such regularity as to avoid the necessity of any artifice for correcting omissions, I find the oscillation detected in 46 monthly means out of 48†; and at Paris, in every one of 132 months‡, the amount there being nearly three times that at Edinburgh.

The influence of the seasons upon the horary oscillation is an important inquiry; and considerable uncertainty prevails in the best observations. With regard to the *hour*, we find the duration both of the morning and evening oscillation is greater in spring and summer than in autumn and winter; 8 or half-past 8 A. M. and 10 P. M. being the critical hours at the former period, and 10 A. M. and 8 P. M. at the latter. With regard to the variable amount, if we take advantage of the double hours of observation, and select the actual maxima, we shall find the following amounts of oscillation:

SEASONS.	MORNING PERIOD.		EVENING PERIOD.	
	Hour.	Oscill.	Hour.	Oscill.
Spring, -	8	.0213	10	.0202
Summer, -	8	.0181	10	.0151
Autumn, -	10	.0136	8	.0074
Winter, -	10	.0031	8	.0031

We thus see that both the morning and evening tide exhibits the same order of decrease through the seasons, descending in winter, to a very minute amount.

* *Astronomische Nachrichten*, 1823, No. 26.

† *Philosophical Transactions* for 1827, &c.

‡ See M. Bouvard's Memoir in the *Mémoires de l'Institut* for 1824.

If we employ the fixed hours of 10 A. M. and 10 P. M. for the maxima, we shall find the oscillation at a maximum in spring and autumn, lower in summer, and the decided minimum in winter. This agrees perfectly with the results of the observations made at the apartments of the Royal Society, in London, which I have collected from the Philosophical Transactions for the last four years, during which the meteorological register has been conducted on a much improved plan.

	SPRING.	SUMMER.	AUTUMN.	WINTER.
	Mar. Ap. May.	June, July, Aug.	Sept. Oct. Nov.	Dec. Jan. Feb.
Mean 4 yrs.*	.0237	.0183	.0314	.0148

That these differences are really owing to the influence of the seasons may be inferred from the excellent agreement of the four total annual means, which are .0227, .0246, .0205, .0214. And we may reasonably conclude, by the analogy of my observations, that, had the true hours of tides been observed, the order in intensity would have been the same. The best observations in other parts of the globe, afford us, it must be admitted, somewhat anomalous results. In the excellent observations of Marqué Victor, at Toulouse, little or no difference from the influence of the seasons was observed.† At Clermont Ferrand the admirable observations of Ramond assure us that the maximum oscillation occurs in spring, and the minimum in winter, ‡ with a secondary maximum and minimum in autumn and summer. At Paris, from eleven years' results, the maximum is also in spring, but the principal minimum in summer, and having a second maximum in autumn. On the Plateau of Bogota, near the equator,§ there are two maxima, which appear nearly to coincide with the maximum intensity of the solar rays, occurring towards the equinoxes. The observations of Dorta, at Rio Janeiro, in 23° S. Lat., give nearly the same results, the maximum being in April, or at the autumnal period in southern latitudes.|| It is remarka-

* For part of those for 1830, I am indebted to Mr Hudson, the observer, whose zeal in the examination of this phenomenon promises soon to afford us new and valuable data.

† *Bibliothèque Universelle*, xx. 246.

‡ *Memoirs de l'Institut*, 1812.

§ Humboldt, *Relation Historique*, iii. p. 302.

|| *Ibid.* iii. p. 298.

ble also, that the observations of Dr Russel at Berhampour (Lat. 24° N.), and of Mr Prinsep at Benares (Lat. $25\frac{1}{4}^{\circ}$ N.), each continued for three years, give, severally, a minimum in summer, and one not so low in winter, with a decided maximum in spring, and a strong indication of a smaller one in autumn.* If, as I think we have good reason to believe, the real maximum is in spring, according to the majority of observations, we may account for this very general apparent maximum in autumn, by considering the effect of change in the hour of tide. For, suppose the barometer observed, as has very generally been the case, constantly at 9 A. M., this corresponds to the true critical hour very exactly at the equinoxes; but in summer, as this hour becomes earlier, the barometer will have passed its maximum at 9 A. M., and the oscillation will come out too small, giving an *apparent* minimum in summer, when perhaps none ought to exist. This, probably, will account satisfactorily for this extensively observed fact, and accords well with the results of my observations at double hours. If we can trust to the reductions of Marqué Victor's observations at Toulouse, they present the greatest anomaly in this part of the subject. The monthly oscillations at the remarkable station of the Grand St Bernard will be noticed shortly.

Without dwelling farther at present upon the deductions from my own observations, I proceed to make some inquiry into the law which regulates the geographical distribution of this remarkable phenomenon.

The general fact that the amount of the barometric oscillation decreases from the equator towards the poles, has been long established. The active researches of Humboldt, between the tropics, above thirty years ago, excited a spirit of inquiry in Europe, in which the distinguished Ramond took the lead, and which terminated in the detection of the horary oscillation among the great accidental variations of atmospherical pressure, at the same hours nearly, but much smaller in amount, than in lower latitudes. The labour of observation was therefore greatly increased, requiring some years of accurate data to afford the desirable precision as to amount, which might be obtained by a few days observation near the Equator. Allowing

* *Philosophical Transactions*, 1828.

for accidental irregularities, the progressive decrease has been very satisfactorily proved ; but two very remarkable registers have, within the last few years, furnished us with unexpected and curious results, which must occupy a prominent place in any theory on the subject. The first of these was the register kept at the convent of the Grand St Bernard, which, as well as a precisely corresponding one at Geneva, was adapted to the investigation of the horary oscillations, immediately after the publication of the third volume of Humboldt's "*Relation Historique*." The St Bernard observations demonstrate, by the regular annual results of the last five years (1826-30,) that, at 8000 feet above the sea, the barometer is *lowest* at 9 A. M. and *highest* at 3 P. M., precisely the reverse of what had hitherto been observed. Humboldt had remarked that, between the tropics, though the amount was diminished, the hour of maximum was not changed. These two results have sometimes been much misunderstood, and spoken of as if they stood in opposition to each other. Mr Daniell (who is almost the only English writer by whom this subject has been considered at any length,) treats the question as one of *quality* not of *degree*. The atmospheric tide is much smaller in Europe, at the level of the sea, than at the same level at the Equator : if, therefore, we consider the influence of height to be equal in both cases, and in the same direction, it is very obvious that while, at the Equator, the oscillation may be only diminished in dimension, at the same height in the Temperate Zone, it may have become null, or changed its sign ; that is, being *negative*, the time of maximum below will correspond with that of minimum above, and *vice versa*,—a phenomenon perfectly in conformity with the laws which ordinarily regulate physical causes, and deducible from the abstract consideration of quantity, therefore to be considered in no respect as an anomaly in the *quality* of the effect, or as a change *per saltum* in the course of nature.

But, what is very remarkable, a precisely analogous fact has been discovered in lat. 74°, by Captain Sir Edward Parry, whose admirable meteorological registers were a most valuable donation to science. This circumstance had for some years been suspected, from the earlier journals of Parry and other arctic voyagers ; but these results were received with just doubt, because the barometric registers wanted the indispensable ele-

ment of the temperature of the mercury, which, under any circumstances, and especially those of an arctic voyage, might produce, by an average difference of temperature at one hour of the day from that at another, results, erroneous in amount, or even opposed to the truth. For example, it appears from my observations, that in the entire annual results, even in this temperate climate, a constant excess of temperature at 4 P. M. was observed above that at 8 A. M., amounting to a degree and a half of Fahrenheit, which, supposing no oscillation, would give rise to a *negative* one of between four and five thousandths of an inch, or nearly half the observed amount in this latitude. It is, therefore, not wonderful that the first observations, which wanted the attached thermometer, should have been received with distrust, and it is rather surprising that Mr Daniell should have so overlooked this source of error, as to have placed the utmost confidence, (in the first edition of his work) in the existence of oscillations which might have been caused by a change of temperature amounting to little more than 1° Fahrenheit; and that he should have published his deduction for the horary oscillation at London from his own observations, which wholly wanted this element. The last voyage of Captain Sir Edward Parry afforded results worthy of the highest confidence, from observations with excellent instruments, of which the indications were registered with an assiduity and precision which puts to the blush anything of the kind, at least in Britain, destined to the furtherance of the science of meteorology. These excellent observations indicate the existence of all the oscillations observed in lower latitudes, including that at 4 A. M., which has rarely been observed in any part of the globe, and give all the values with a negative sign, relatively to the ordinary oscillations. The six monthly means (Nov. 1824—April 1825) give *every one* the negative tide from 4 A. M. to 10 A. M., and likewise from 10 A. M. to 4 P. M., and from 10 P. M. to 4 A. M.; and every month but one from 4 P. M. to 10 P. M. As the greatest observed tide was that from 4 A. M. to 10 A. M., or $-.0089$ inch, I shall adopt it in future computations, more especially as from three months' observations, when the daily number was extended to twelve, the critical hour in the afternoon appeared

to be later than 4, perhaps considerably. * These observations, then, establish the existence of an oscillation in lat. 74° , nearly equal to that in lat. 56° , and affected by an opposite sign.

The number of observations in different parts of the globe being now very considerable, it appeared to me of high importance, in the present state of the science of meteorology, to endeavour to generalize them, and see how far they might be represented by an empirical law. I was accordingly engaged in classifying observations collected from every quarter, when I was fortunate enough to meet with an abstract of a very important memoir, by M. Bouvard, of the observatory of Paris, upon this subject, and read recently at one of the annual meetings of the Helvetic Society. It is upon the hourly variations of the barometer; and the only abstract of it I have met with is that contained in the *Bibliothèque Universelle* for 1829, nor have I seen it in any other periodical work. M. Bouvard, whose important contributions to meteorology are universally known, has here amassed a great addition to the observations collected by Humboldt; and he undertakes the bold enterprize of representing the extent of the oscillation in any latitude, at any height above the sea, and at any period of the day or year, by an arbitrary formula: but though his table presents perhaps more accordance with observation than might have been expected from so sweeping a generalization, I think there is much reason to question the accuracy of the formula, which is founded on these conditions;—that at the equator the extent of the oscillation is proportional simply to the temperature, on the centigrade scale, of the period during which the oscillation is observed at the given spot, the oscillation and temperature at the level of the sea being unity;—that in any other latitude, the same law is to be modified by introducing the additional proportionality to the square of the cosine of the latitude. Or, representing by m and t the tide and mean temperature at any place and for any period, in latitude θ ; and by m' and t' those quantities at the equator, M. Bouvard gives $m' = \frac{t' m}{t \cos^2 \theta}$.

* A similar fact with regard to Northern Europe has been suggested by some observations made at the apartments of the Royal Society of London, in a paper by Mr Lubbock, V.P.R.S. in the *Phil. Trans.* for 1831, of which the author has been kind enough to favour me with a copy since this paper was read.

TABLE of the First Period of the Hourly Variations of the
M. BOUVARD's Formula.—Equatorial Temp. = 30° Cent.

OBSERVERS.	LOCALITIES.
Duperrey.	Offak, 3 days observation, - -
Freycinet.	Rawak, 4 days. - -
La Condamine.	Quito, doubtful observations. -
Humboldt.	Antisana, a single day, weather unfavour.
Caldas.	Popayan, 4 days. - -
Jacquín Purera.	St Louis Maranhán, a year, Engl. Bar. } without thermometrical correct. (a) }
Humboldt.	Ibague, 3 days, - -
Caldas.	Santa Fé de Bogota, 19 successive months,
Boussingault.	Idem, by more than 1000 observations,
Duperrey.	Payta, 5 days, (b) - -
Idem.	Ascension (Isle) 3 days, 6 A.M. to 6 P. M.
Freycinet.	Coupang, 9 days, - -
Humboldt.	Cumana, 12 days, - -
Idem.	Caracas, 11 days, - -
Idem.	Guayra, 11 days, - -
Idem.	Lima, 4 days, - -
Idem.	Callao, 3 days, - -
Simonoff.	O-Taïti, 7 days, - -
Humboldt.	Mexico, 3 days, - -
Freycinet.	Port Louis, Isle of France, 20 days, -
Duperrey.	Idem, 4 days. - -
Freycinet.	Rio Janeiro, 2 days. - -
Dorta.	Idem, 12 months. - -
Russell.	Berhampour (India), 3 years. -
Prinsep.	Benares, 3 years. - -
Coutelle.	Cairo. 25 days. - -
Freycinet.	Port Jackson, 19 days. - -
Gambart.	Marseille, 5 years. - -
Marqué Victor.	Toulouse, 4 years. - -
Valz.	Nismes, 1 year. - -
D'Hombres Firmas.	Alais, 3 years. - -
Gasparin.	Orange, 5 years. - -
Billiet.	Chambery, 18 months. -
	St Bernard, 5 years (c). - -
Ramond.	Clermond-Ferrand, 7 years. -
	Geneva, 3 years. - -
	Bevers (Switzerland). - -
Fueter,	Berne, 10 years. - -
Horner.	Zurich, 1 year. - -
	St Gall, 1 year. - -
Herrenschneider,	Strasburg, 12 years. - -
Bouvard aîné.	Paris, 12 years. - -
Nell de Bréauté.	La Chapelle 8 years. - -
Crahay.	Maëstricht, 9 years. - -
Royal Society,	London, 1 year (g). - -
Sommer.	Königsberg, 8 years. - -

Barometer, reduced to the Equator at the Level of the Sea, by

Height in Metres.	Latitude.	Temp. Centigr.	Oscillations observed.	Reduced to Equator.	REFERENCES.
			Millim.	Millim.	
10	0 2	28,0	2,93	3,14	From MS. obs.
10	0 2	26,0	2,61	3,01	Ditto.
2907	0 14	16,5	2,25	4,09	Humboldt.
4093	0 33	8,5	0,97	3,42	Ditto.
1776	2 26	18,0	2,07	3,45	Ditto.
10	2 30	29,0	3,79	3,93	Orig.
1370	4 28	21,0	2,59	3,72	Humboldt.
2660	4 36	17,0	2,10	3,73	Ditto.
—	—	17,0	2,80	4,09	Ditto.
10	5 6	27,0	2,66	2,98	MS. obs.
10	7 55	28,0	2,43	2,72	MS. obs.
10	10 9	28,0	2,96	3,27	MS. obs.
10	10 28	26,0	2,55	3,05	Humboldt.
936	10 31	21,0	2,70	3,99	Ditto.
10	10 36	26,5	2,75	3,22	Ditto.
166	12 3	23,0	2,77	3,77	Ditto.
10	12 3	20,0	2,22	3,48	Ditto.
10	17 29	24,0	2,13	2,95	Zach, Corr. Astr.
2231	19 26	17,0	1,80	3,57	Humboldt.
10	20 10	22,0	1,72	2,66	MS. obs.
—	—	24,0	1,92	2,73	MS. obs.
10	22 54	24,2	2,58	3,77	MS. obs.
—	—	—	2,33	3,39	Humboldt
.....	24 4	24,0	2,23	3,42	Phil. Trans.
.....	25 30	26,0	2,69	3,81	Ditto.
.....	30 3	22,0	1,89 (c)	3,44	Voyage d'Egypte.
10	33 51	21,0	1,71	3,52	MS. obs.
46	43 17	16,0	0,83	2,94	Conn. des Temps.
156	43 36	15,0	1,00 (d)	3,81	Acad. Toulouse.
65	43 50	16,5	0,98	3,42	Orig.
132	44 7	16,5	0,99	3,49	Ditto.
.....	44 8	16,5	0,85	3,00	Ditto.
267	45 34	14,0	1,00	4,37	Acad. Chambery.
2491	45 42	-0,7	+0,046	4,04	Bibl. Univ.
410	45 46	12,5	0,94	4,53	Mem. de l'Institut.
407	46 12	12,8	0,74 (f)	3,62	Bibl. Univ.
1657	46 34	7,5	0,45	3,81	Orig.
532	46 57	12,0	0,90	4,82	Bibl. Univ.
405	47 22	13,0	0,88	4,43	Ditto.
635	47 26	11,0	0,56	3,34	Ditto.
150	48 35	13,0	0,88	4,29	Orig.
65	48 50	13,8	0,76	3,81	Mem. de l'Institut.
154	49 49	11,5	0,49	3,07	Orig.
52	50 51	13,0	0,57	3,30	Quetelet.
30	51 29	12,6	0,57	3,50	Phil. Trans.
30	54 42	6,0	0,20	3,27	Humboldt.

NOTES TO THE PRECEDING TABLE

(a) On account of the want of an attached thermometer, this series ought to be discarded.

(b) Modified from the result given by Humboldt, *Rel. Hist.* iii. 312.

(c) This amount, somewhat different from that given by Humboldt, appears to be the result of a new analysis of the original observations in the "*Voyage d'Egypte*."

(d) This is *smaller* than was originally assigned by Marqué Victor.—See p. 278. *Note*.

(e) These are the first five of the years given in p. 274, with the exception of 1821. By a more extended and systematic reduction from the Table there given, I find the amount to be $+ .009$ lines $= 0^{\text{mm}}.020$.

(f) There is probably some mistake in assigning 0.74 as the amount at Geneva. For the years 1826–8, to which M. Bouvard must probably have referred, it was $0^{\text{mm}}.86$; and the mean of five years, 1826–30, gives $0^{\text{mm}}.81$.

(g) It will be seen by p. 267, that the mean of four years gave $.0223$ inch. $= 0^{\text{mm}}.566$.

By the formula above-mentioned, the column of calculated results in the table I now subjoin has been computed, the temperatures in which, it is to be observed, are not the mean temperatures of the place, but of the hours included in the great period from 9 or 10 A. M. till 3 or 4 P. M. I must also remark, that where the number of daily observations was considerable, M. Bouvard has made use of a formula depending on the sine of the diurnal arc. The application of this formula is very briefly noticed in the abstract to which I allude; but it appears to have been almost entirely confined to intertropical observations, where the oscillation has frequently to be eliminated from the irregular observations of a few days. In northern latitudes I have generally found the amount unaltered from the original announcement in the various publications to which these scattered results have been confined. I have added to this valuable table, which contains many results previously unpublished, a few remarks as they occurred to me.

I have now some more particular observations to add upon the theoretical part of the table. 1st, I conceive that it is little to be expected, or perhaps desired, that one formula should represent the decrement both in latitude and height, and the change at different hours and seasons, which, even in the much better known facts of terrestrial temperature, has never been,

nor satisfactorily could be, executed. I conceive that, in the purely physical bearings of the question, the existence of such a common law would be surprising, because such a coincidence must have been derived from the concurrence of independent agencies. 2d, The observations at considerable heights in different latitudes, are too few, too brief, and too inconsistent, to afford us any thing like satisfactory results. For example, the observations at Caracas, at a height of 3000 feet, give a greater amount of oscillation than those at Cumana, at the level of the sea, at the same latitude. 3d, M. Bouvard proposes his formula as capable of expressing the variation observed at any period of the day or year, and has shewn that the ratio of the different mean temperatures of the periods of forenoon and evening oscillations, correspond tolerably nearly with that of the observed amount of these oscillations, at least in Europe; but I would ask, why do not the great differences of temperature, corresponding to the seasons, give the marked results which his formula would suppose, producing a tropical amount in summer, and an arctic one in winter? At Paris, for instance, where the mean of the summer temperatures, that is, of the months of June, July, and August, is, according to Humboldt*, 18°.1 Cent.; and of the winter months (December, January, and February,) 3°.7 Cent., the ratio is 4.9 : 1; whilst the corresponding oscillations, which, according to M. Bouvard's formula, ought to have the same ratio, are 0.^{m.m.}766 and 0.^{m.m.}685, or as 1.1 : 1,—this ratio being less than one-fourth of the other. Yet the Paris observations for the barometric tide are among the best we possess; and these numbers are a mean from the registers of eleven years. Again, at the very important station of the Grand St Bernard (to which M. Bouvard strongly appeals in support of his formula,) where the mean result is negative with regard to the oscillation at the level of the sea, the mean temperature of the year being below 0° Cent., therefore rightly expressed in the preceding table, we should have a very striking difference of result in summer and winter. The temperature of the three summer months being about + 6° Cent., and of the three winter months about — 6° Cent. the first should give a considerable *positive* oscillation, the

* Des Lignes Isothermes. *Mem. d'Arceuil*, tom. iii. *Table*.

latter a considerable *negative* one. Let us see, therefore, how the case stands. For this purpose, I shall classify under the seasons the whole of the monthly results which have been published, containing fifteen months' observations, in 1821, 1822, and 1823, and the years from 1826 to 1830, inclusive, which are complete. The sign of + indicates a rise between 9 and 3, that of — a fall: the former, therefore, is to be considered *negative* with regard to the ordinary course, since the barometer rises when it usually falls at the level of the sea. I may add, that, by taking the mean temperature of the day in place of that of the particular period in computing the oscillation by M. Bouvard's formula, here and at Paris, we are doing it no injustice, as, though the summer and winter temperature would both be somewhat higher*, the *ratio* would be almost the same; and it is about no evanescent differences that we are disputing.

Convent of the Grand St Bernard.

	Spring.			Summer.			Autumn.		Winter.	
	Mar.	Ap.	May.	June,	July,	Aug.	Sept.	Oct.	Nov.	Dec. Jan. Feb.
Mean,	+0.018			+0.080			—0.033		—0.029	

I may add, the individual results justify perfectly the deduction of the mean, there being only *three* — results in the summer period, and only *five* + in the winter one; therefore, instead of an oscillation corresponding to the Temperate Zone in summer, and to the Arctic Regions in winter, we have precisely the reverse. It is hardly necessary to remark, how totally opposed this is to the theory of M. Bouvard. †

I would remark, *fourthly*, upon M. Bouvard's formula, that, by fixing the mean temperature from which he calculates his numerical results at the mean of the period from 9 A. M. to 3 P. M., he has given himself certain limits of which it is almost

* Indeed there seems much reason to doubt whether the period of the day alluded to has a mean temperature below 0° Cent. If it has not, M. Bouvard's formula would totally fail, as the equatorial oscillation would come out with a wrong sign.

† In 1829, the mean temperature of the seasons, distributed as above, was — 1°.5 R. + 4°.8 R., + 0°.5 R., and — 4°.6 R. In 1830, it was colder, as follows: — 0°.1 R. + 4°.5 R., — 0°.7 R., and — 7°.7 R. respectively.

impossible for a speculator not to avail himself, and which must be at all times unsatisfactory on account of the great want of the thermometrical observations for fixing the mean temperature of any particular portion of the day in so many latitudes. I have to observe, in the *last* place, that the results are far from satisfactory even when the difficulties of the subject are considered. By consulting the last column of the table already given, we find the deduction of the formula for the oscillation at the Equator range from 2.72 to 4.82 millimetres; and, what is worse, that the + and — errors from the mean are not well balanced throughout the quadrant of latitude, but that there is a general rise in their value as we proceed farther from the equator. As it is undeniable that the tropical observations, from the smallness of the reductions required, afford the most correct results, I am strongly of opinion that the mean of his numbers would give us an equatorial oscillation greatly above the truth. By looking over M. Bouvard's table, we shall find not a single authentic series of observations gives an oscillation of three millimetres (we except the observations at St Louis Maranhan, which, wanting the results of the attached thermometer, should be discarded), the maximum being 2.93, which occurred at the very equator. Humboldt considers 3.3 millimetres the extreme limit for the equator; and I believe there is no series in existence, continued for several days together, which gives such a mean result. The observations of Duperrey, at Payta, which, by Humboldt's table, was reckoned at so extreme an amount as 3.4, has dwindled, by the analysis of M. Bouvard, to 2.66, which is, in all probability, a very correct result; and my own opinion is, that future observations will limit the equatorial oscillations to 3 millimetres, or perhaps even lower, except in anomalous situations. Now, the result of the table we are discussing, generally gives an oscillation nearer 4 than 3 millimetres; but what is remarkable, the larger numbers occur only where the amount of reduction is considerable. Thus, confining ourselves to observations not at extraordinary heights above the level of the sea (that is, under 2000 metres), we do not find a single observation under latitude 45°, affording an equatorial oscillation amounting to 4 millimetres; whilst between 45° and 55°, in less than half the

number of localities, we have no less than five results above 4 millimetres. This alone indicates something wrong. Had M. Bouvard limited his passion for generalization to the law of *latitude* only, he would easily have given a nearer approximation; this was what I had proposed to undertake before I had the good fortune to meet with his paper, and what I afterwards executed with the aid of his data.

As M. Bouvard has presented us in his table with a considerable number of unpublished results, which I was unwilling to lose, I have trusted to the skill and fidelity of the author in deducing the numbers which he has given as the *observed amount* in his table. From observations continued but a few days, and at irregular hours, it requires some labour and judgment to obtain the truest results. It is therefore almost entirely in equatorial results that M. Bouvard differs in his numbers from the quantities already assigned by such observers as have published their registers, and from a very careful and extended comparison which I have made with the originally published synopses of observations made in the Temperate Zone, I rarely find any change made upon them.* The table given in p. 272, has therefore been my guide, as far as absolutely observed results are there given; and it would be unjust not to add here, that M. Bouvard deserves the thanks of all who take an interest in the subject, for the spirited and valuable paper from which the table is extracted, and for the unpublished or scattered results which he has there combined, as well as that his particular theory is proposed with much diffidence.

As my investigation extends merely to the law of diminution

* The principal deviation which I have remarked is in the extensive series of M. Marqué Victor, at Toulouse, where the observations were made many times a-day, and probably had M. Bouvard's formula of reduction for hours applied to it. However, it seems rather unaccountable how any formula of this kind should *diminish* instead of increase the amount of oscillation, which it actually does in this case, unless there be some numerical mistake, the amount derived by M. Marqué Victor, (and which I have verified as far as the combination of the annual means is concerned) being $1^{\text{mm}}.20$; according to M. Bouvard it is $1^{\text{mm}}.00$. As, however, M. Bouvard treats this as the best determined point in Europe, it is probable that he had carefully examined the tables in the *Memoirs de l'Academie de Toulouse*, the later volumes of which I have not been fortunate enough to see.

connected with latitude, I have confined myself to observations near the level of the sea. The greatest exception is the result of M. Ramond at Clermont-Ferrand, at the height of 410 metres; but the excellence of the observations is such, and they agree so well with those in the same parallel, that I could not omit them. I have added the value given by Captain Sir Edward Parry's observations for Port Bowen, and my own for the neighbourhood of Edinburgh. My object being to determine the law of variation with latitude which should best represent a given number of trust-worthy observations of the horary oscillation, I have employed a mode of investigation similar to that used by Mr Atkinson, in his analysis of the law of mean temperature, forming part of his elaborate paper upon Astronomical Refractions, inserted in the second volume of the *Memoirs of the Astronomical Society*.

I assumed the form of the function of the latitude which expresses the oscillation, to be some unknown power of the cosine, affected by an unknown coefficient, and constant. These quantities are therefore to be eliminated from three or more equations, derived from observations in different parts of the globe. Let the barometric oscillation be represented by z, z'', z''' , in latitudes $\theta, \theta'', \theta'''$ respectively: then, denoting by n the unknown power of the cosine, and by α and ϵ the other constants, we may separate the first of these unknown quantities from three equations of the form $z = \alpha \cos^n \theta + \epsilon$, by putting them under that of $\frac{z, - z''}{z, - z''} = \frac{\cos^n \theta, - \cos^n \theta''}{\cos^n \theta, - \cos^n \theta''}$. Whence the value of n may be deduced by approximation. For this purpose, I selected nine values of z from the list of observed oscillations, and, for greater convenience of calculation and tabulation, I shall employ these during the rest of this paper in the form of *millimetres*, which in fact, since Humboldt wrote, have become the most common units of measure for these results. These numbers were not in general single results, but were combined from several in such a way as appeared best to correct accidental irregularities. Three observations nearly equatorial were taken, three towards the tropics, and three in high latitudes, as follow:

	Lat.	Oscill.		Lat.	Oscill.
		m.m.			m.m.
Ascension,	7 55 S	2.43	Cairo,	30 3 N.	1.89
Coupang,	10 9 S.	2.96	Port Jackson,	33 51 S.	1.71
			Marseilles,	43 17	0.83
Mean,	9 2	2.70	Toulouse,	43 36	1.00
Cumana,	10 28 N.	2.55	Strasburg,	48 35	0.83
La Guayra,	10 36 N.	2.75	Paris,	48 50	0.76
Mean,	10 33	2.65	Mean,	46 4	0.86
Lima,	12 3 S.	2.77	Maëstricht,	50 51	0.57
Callao,	12 3 S.	2.22	London,	51 29	0.57
			Königsberg,	54 42	0.20
Mean,	12 3	2.49	Edinburgh,	55 55	0.27
Isle of France,	20 10 S.	1.72			
Rio Janeiro,		1.92	Mean,	53 14	0.40
		2.58			
	22 54 S.	2.73	Port Bowen,	73 48	-0.22
Mean,	21 22	2.14			

Selecting nine of the possible combinations by threes, we obtain the following values for n :

Lat.	9° 2'	} $n = 5.0$	Lat.	10 33	} $n = 4.9$
	21 22			21 22	
	46 4			46 4	
Lat.	9 2	} $n = 1.8$	Lat.	12 3	} $n = 1.8$
	38 3			33 51	
	53 14			73 48	
Lat.	9 2	} $n = 2.2$	Lat.	12 3	} $n = 3.1$
	33 51			21 22	
	73 48			46 4	
Lat.	10 33	} $n = 1.6$	Lat.	12 3	} $n = 0.8$
	30 3			30 3	
	53 14			53 14	
Lat.	10 33	} $n = 2.2$			
	33 51				
	73 48				

Mean value of n , = 2.60, or about $\frac{5}{2}$.

In order to modify the co-efficient and constant in the arbitrary formula, so as best to agree with observation, we shall select a series of observations from M. Bouvard's table, which,

from their intrinsic merit, or from their representing accurately a group of minor observations, shall seem best fitted to give accurate results. We shall first take an approximate value of α and ϵ from the observations at Cumana and Toulouse, which represent pretty nearly the whole of the rest. From them we obtain the two following equations:

$$2.55 = \alpha \cos^{\frac{5}{2}} 10^{\circ} 28' + \epsilon = .959 \alpha + \epsilon,$$

$$1.00 = \alpha \cos^{\frac{5}{2}} 43^{\circ} 36' + \epsilon = .446 \alpha + \epsilon.$$

$$\text{Whence } \alpha = 3.02; \epsilon = -0.35.$$

Substituting these values in the expressions for the selected oscillations, we obtain the approximations contained in the 4th column of the following table, upon which it is well worthy of remark, that, by means of the constants derived solely from the employment of observations in latitudes $10^{\circ} 8'$ and $43^{\circ} 36'$, which have both a sign of +, we obtain a result for lat. $73^{\circ} 48'$ N. not merely giving a sign of —, but agreeing in amount with singular precision with that which observation affords, proving that a *negative* oscillation, instead of being in any way anomalous, is nothing else than we might have inferred *a priori* for high latitudes.

PLACE.	Latitude.	Oscillation.			Oscill. Final Calc. Millim.	Least Error. Millim.
		Obs. Millim.	Approx. Millim.	Calc. Error. Millim.		
Payta,	5 6 S.	2.66	2.64	—0.02	2.620	—0.04
Cumana,	10 28	2.55	2.55	0.00	2.525	—0.02
Otaheité,	17 29 S.	2.15	2.33	+0.18	2.312	+0.16
Isle of France,	20 10 S.	1.82	2.23	+0.41	2.206	+0.39
Rio Janeiro,	22 54 S.	2.45	2.11	—0.34	2.087	—0.36
Cairo,	30 3	1.89	1.76	—0.13	1.732	—0.16
Toulouse,	43 36	1.00	1.00	0.00	0.971	—0.03
Clermont,	45 46	0.94	0.88	—0.06	0.851	—0.09
Paris,	48 50	0.76	0.71	—0.05	0.685	—0.07
Maestricht,	50 51	0.57	0.61	+0.04	0.579	+0.01
Königsberg,	54 42	0.20	0.42	+0.22	0.388	+0.19
Edinburgh,	55 55	0.27	0.36	+0.09	0.332	+0.06
Port Bowen,	73 48	—0.22	—0.23	—0.01	0.256	—0.04

In order that the constants may express the general result

of all these thirteen observations in the best possible manner, we must reduce the sum of the squares of the errors in the 5th column to a minimum by the method of Legendre. By this means we obtain the formula

$$z = 3.031 \cos^{\frac{5}{2}} \theta - .381 \text{ for millimetres,}$$

$$= .1193 \cos^{\frac{5}{2}} \theta - .0150 \text{ for English inches.}$$

by which the numbers in the 6th column are calculated.

Since the oscillation becomes negative in the higher latitudes, there is some point at which it changes its sign, or where no oscillation occurs. This latitude, which may be denoted by L , is readily found by equating the value of z to zero, which gives

$$\cos L = \left(\frac{.381}{3.031} \right)^{\frac{2}{5}}, \text{ and } L = 64^{\circ} 8' 6''$$

By putting $\theta = 0$ and $\theta = 90^{\circ}$, we obtain for the equatorial oscillation, $2^{\text{mm}}.650$; and for the polar, $-0^{\text{mm}}.381$; or .119 and .015 English inches.

By means of the formula $z = a \cos^n \theta + c$ (putting for a and c the corrected numbers,) we may farther deduce the mean amount of the atmospheric tide for the quadrantal arc of the meridian, and likewise corresponding to the entire surface of a hemisphere. In the first case, its mean value will correspond to the integral of $z d\theta$, taken between the limits $\theta = 0$ and $\theta = 90^{\circ}$, and divided by the length of the quadrant of latitude, or

$$\frac{2}{\pi} \int (a \cos^n \theta + c) d\theta.$$

In the second case, we must introduce the expression for the length of the parallel of latitude, and divide the integral taken within the same limits as before, by the *surface* of the hemisphere, that is,

$$\frac{\int 2\pi a \cos^{n+1} \theta d\theta + \int 2\pi c \cos \theta d\theta}{2\pi} = \frac{\int (a \cos^{n+1} \theta + c \cos \theta) d\theta}{\int \cos \theta d\theta}.$$

Since we have employed $n = \frac{5}{2}$, the integration of both these

expressions comes under that of elliptic transcendentals; by a simple alteration, however, we shall obtain a direct solution, abundantly accurate for our present purpose. We have seen that $n = 2.6$; if, therefore, we make it $= 3$, instead of $\frac{5}{2}$, and modify α and ζ , we may obtain a different expression, which shall very closely represent the observations. We shall find for the new equation

$$z = 3.07 \cos^5 \theta - .36 \text{ as a near approximation.}$$

The first of the above integrals then becomes

$$\frac{2}{\pi} \int (3.07 \cos^5 \theta - .36) d\theta,$$

which, taken between the limits $\theta = 0$ and $\theta = \frac{1}{2} \pi$, gives for the mean oscillation, in relation to the quadrant of latitude,

$$\frac{4.09}{\pi} - .36 = \overset{\text{m.m.}}{0.94}.$$

The other integral above given becoming

$$\int (3.07 \cos^4 \theta - .36 \cos \theta) d\theta,$$

there results, within the same limits,

$$3.07 \cdot \frac{3}{16} \pi - .36 = \overset{\text{m.m.}}{1.45}$$

for the mean value with regard to the surface of a hemisphere.

These numbers correspond to homogeneous columns of air at the ordinary pressure and temperature $10\frac{1}{2}$ and 16 metres in height respectively. In the investigation of any supposed connection with temperature, these mean results will be of some value.

I am satisfied for the present with having pointed out a formula representing very closely the observed amount of oscillation at the level of the sea, as depending upon latitude. For any successful generalization upon the influence of height, we must wait for vastly more extended data than we already possess; and the same remark is applicable, though to a less extent, to the influence of the seasons. That temperature has an important connection with the geographical distribution of this phenomenon, I have no doubt. It appears probable, that,

among places under the same latitude, and having different mean temperatures, the coldest has the smallest oscillation.* We know that in ascending above the level of the sea, they diminish together, the curve of temperature being probably asymptotic, whilst that representing the oscillation would appear to cross the axis at a certain height. At present, I have confined myself to arriving at a generality of the first degree; the higher degrees, which will embrace the element of temperature, will probably go far towards an explanation of the *cause*, with which that element is certainly nearly concerned. But we must be contented to wait in the meantime for additional data.

I have done perhaps as much as can be expected from a solitary observer towards fixing the minute quantity corresponding to this latitude. It is from public and learned bodies alone that we can look for registers of perfect regularity, combined with precision as to the hours of observation. I look with sanguine expectations towards the establishment of such a one by this Society. Strange though it be, I believe we may safely affirm, that Great Britain does not at this moment produce a register worthy of the present advanced state of meteorology. Scotland, by her geographical position, is well situated for unfolding many important phenomena of Nature, and, amidst the disadvantages of her inconstant sky, offers some peculiar recommendations to the zealous observer both in meteorology and magnetism; but of these it has been her misfortune to meet with few or none. But the spirit must be fostered by her societies. “*La simultanéité et la durée,*” says an accomplished French philosopher, speaking of these bodies, “*que leur institution donne à des efforts mortels, complètent la puissance de la méthode expérimentale. Elles seules pouvaient désormais assurer la continuité du progrès des connaissances humaines;*

* Thus Königsberg, though in a lower latitude, appears to have a smaller oscillation than Edinburgh; but then the temperature appears to be only 43° F. (*Astronomische Nachrichten*, Feb. 1823,) while that of Edinburgh is 47°. It must be hardly necessary here to observe, that, supposing the mean temperature of a place known in a function of the latitude, my formula admits of a direct comparison with the temperature (t) at the level of the sea. Thus, if Dr Brewster's formula of $t = 81.5 \cos \theta$

be employed, my formula becomes $z = a t^{\frac{5}{2}} - c$; a being a new coefficient.

seules elles pouvaient développer les grandes théories, et faire obtenir des résultats qui, par leur difficulté, par la diversité, la persévérance, et l'étendue des travaux qu'ils exigent, n'auraient jamais été accessibles pour des individus." *

I cannot help remarking, in conclusion, that this observation was never more completely verified than in the hourly thermometrical observations made for some years together under the auspices of the Royal Society of Edinburgh, at the suggestion of its late learned Secretary Dr Brewster, which, I hesitate not to affirm, is the noblest donation ever made towards the progress of meteorological science.† Valuable, not merely from the specific results, important as they are, which it afforded, but even more so, as demonstrating the susceptibility of the science to assume a mathematical form, and proving that the confused obscurity which so long overhung the laws of mean temperature was due to the imperfections of our mode of observation, not to any anomaly in Nature itself, which experience daily more firmly convinces us is governed by laws equally immutable, whether palpable to the senses, or veiled by an indefinite series of secondary causes.

Since this memoir was read, I have been favoured with a confirmation, particularly interesting, of the identity of the law which regulates the horary oscillation in the northern and the southern hemisphere. Captain Philip P. King, R. N. permits me to make use of the results deduced from his admirable MS. register kept for six months together, at Port Famine, in the Straits of Magellan, South lat. $53^{\circ} 38'$; West Long. $70^{\circ} 54'$. The following numbers give the difference of height in the barometer, reduced to 32° F. between 9 A. M. and 3 P. M. for each month ‡ :

* Biot, Notice des opérations entreprises pour déterminer la figure de la terre.

† The results are published in the Society's *Transactions*, vol. x.

‡ The abstracts of Captain King's observations are now published in the *Proceedings of the Royal Geographical Society*, vol. i.

1828, February,	-	-	— .016
March,	-	-	— .008
April,	-	-	— .026
May,	-	-	— .026
June,	-	-	— .006
July,	-	-	— .042
Mean Oscillation,	-		.0207
Calculated by the Formula,			.0173
Error,			— .0034

The temperature was so low (not exceeding 43° F. for the period from 9 A. M. to 3 P. M.) that M. Bouvard's formula would err very greatly in defect.

ART. IX. — *Account of the Common Mackerel, (Scomber scombrus, Lin.) and the Garuin of the Ancients.* By BARON CUVIER and M. VALENCIENNES.

THE common Mackerel belongs to the tribe *Scomberoides*, which includes the tunny and the bonito, the fishery of which employs much capital and many hands. The general form of the mackerel is too well known to require particular description. Its colours are particularly brilliant; a fine steel blue on the back, changing into golden green and purple, and waved with black lines to a little below the lateral line. The under part of the body is of a silvery white, with purple and golden reflexions. It has no swimming-vessel.

It is generally believed that the mackerel is a migratory fish. Next to the herring, it affords the most abundant and lucrative fishery in the seas which wash the north-west of Europe. The route which Anderson has traced of the migrations of the mackerel is well known. This fish (says he) passes the winter in the north; towards the spring the great shoal coasts along Iceland, Scotland and Ireland, and throws itself into the Atlantic Ocean, where one column, in passing along Portugal and Spain, enters the Mediterranean. The other column enters La Manche, appears in May on the coasts of France and England,

and passing from thence is found on the shores of Holland and Friseland in June. This second column, having arrived in July on the coasts of Jutland, detaches a division, which, making the tour of this almost island, penetrates into the Baltic Sea, and the remainder, passing along Norway, returns to the North. But Mr Anderson throws doubts upon a recital, in itself very improbable, when he adds, that the mackerel, not being an object of commerce, and exciting but little attention, he could only obtain this information from two fishermen of Heligoland.

Other fishermen, referred to by Duhamel as his authorities, relate that the mackerels pass the winter in the different bays of Newfoundland; that they bury themselves in the mud, where they remain till the end of May, when the melting of the ice permits them to appear in great numbers along the coasts, and when many are taken. At this time, however, they have, it is said, a taste of mud, and it is in July and August only that they are fat and well tasted.

Admiral Pleville-Lepley, who had his home on the ocean for half a century, communicated an observation to M. Lacépède, which seems to confirm this story. He assured him that at Greenland, in the small bays surrounded with rocks, so common on this coast, where the water is always calm and the bottom generally soft mud and fuci, he had seen in the beginning of spring myriads of mackerels with their heads sunk some inches in the mud, their tails elevated vertically above its level, and that this mass of fishes was such that at a distance it might be taken for a reef of rocks. The Admiral supposed that the mackerels had passed the winter torpid under the ice and snow; and added, that, for fifteen or twenty days after their revival, these fishes were affected with a kind of blindness, and that then many were taken by the net; but as they recovered their sight the net would not answer; and hooks and lines were used. Something similar to this is found in Schonevelde's *Ichthyology*. Certain seamen had related to this author, that at the end of autumn there grows over the eyes of the scombers a pellicle like a nail, which blinds them during winter, and which falls off or disappears in spring. This is said to be the reason that mackerels are earlier taken on the southern coasts, and that they are not fished in winter.

It is not impossible, in point of fact, that the adipose skin which is narrowed before and behind the orbit of the mackerel, may take in winter more breadth and thickness, and cover the greater part of the eye. But the sojourn of the mackerel in the creeks of Greenland, in a state of torpidity, is the more to be doubted, as Otho Fabricius, who lived so long in this country, does not even name the mackerel as one of the fishes of the coast.

This much is certain, however, that there are found on the coasts of La Manche in the month of April young mackerels without milts, which are called in Normandy *sansonnets*, and in Picardy *roblots*. Towards the end of May mackerels are in roe, and they are taken in this state in abundance all the month of June and part of July. In the month of August, after spawning, they are termed *chevillés*; and about the end of September and in October small ones only are caught, which appear to be the young of the year. But all this is very irregular; and it is not uncommon to see at Paris mackerels taken at Dieppe in the months of November and December. Their unexpected appearance on the coasts at this time has been attributed to the effect of tempests at sea, which, if true, would prove that they do not retire so far to the north as is alleged.

Duhamel states, like Anderson, that the shoals of mackerel enter La Manche from the west, and follow a route contrary to that of the herrings; though he assures us in the same page that the fishermen of Dunkirk take them before those of Dieppe and Havre; and a little farther on, that the Yarmouth fishery precedes that at the entrance of La Manche. According to Low, great shoals appear among the Orkney islands in the end of July and beginning of August.

Schonevelde says that the mackerel is almost unknown on the western coasts of Holland, and that a few are only taken around the island of Heligoland; but he acknowledges that they are found in the Baltic. They would even seem to spawn in that sea, for the fishermen of Ecreford, on the east side of Holstein, call the young mackerels of the length of a palm *prieglers*.

What disposes us most to doubt the truth of Anderson's

narrative is, that the fishery of the mackerel commences in the Mediterranean at the same time as in the North Sea and in La Manche, and even sooner. This fish is taken at Aiguemortes from April to August. All along the coast of Languedoc, the fishing season is in June, July, and August. At St Tropez and at Frejus in Provence, they are caught from the month of May sometimes even till October; and M. Risso assures us, that in spring mackerels are taken abundantly on the coast in the neighbourhood of Nice. Even in the Black Sea, along the coasts of Taurida, they appear in spring and during summer in great shoals, of which all the individuals, even the small ones, are in full milt or roe. They come from the west, and the sea-birds, attracted by the lustre of their colours, follow their tract, and prey upon them. They do not, however, enter the sea of Azof. To the south the mackerel is found beyond the Straits of Gibraltar; for there are specimens in the museum which Adanson brought from the Canaries.

It appears that the mackerel varies in size and taste, not only according to the season, but also according to the places where it is taken. In the Baltic, where it does not exceed a foot in length, little account is made of the fishery. Allamand and Lefranc de Berkhey, quoted by Duhumel, assure us that the fish is not much esteemed at Amsterdam; Pennant says it is not of much use because it will not bear carriage, and that few are salted but in Cornwall, where it furnishes food in this state for the poor; while, according to Anderson, the Icelanders despise it, and do not give themselves the trouble to take it. These assertions will seem very strange to the inhabitants of Paris, to whom this fish furnishes during summer an article of food so abundant and agreeable. It would appear to result from this, that it is along the coasts of La Manche that the mackerel arrives at its greatest perfection. It is at the entrance of La Manche, between Sorlingues and the l'Ile de Bas, that the largest mackerels are taken. They are there found nearly two feet long; but the smaller ones are preferred to be eaten fresh. In the Mediterranean, generally, the mackerel is small and dry, and is supposed to be inferior to that of the ocean; but I suspect that this has arisen in part from the fishermen having taken, in place of the true mackerel, the two species with

swimming-bladders, of which we shall afterwards speak. M. Risso, who distinguishes the two species, says that the flesh of the mackerel is agreeable, but that it never weighs above four pounds; Cetti assures us, that in Sardinia, where it is called *pisaro*, although not fished extensively, it is regarded as a very excellent fish. On the coasts of the Black Sea, the fishery most prosecuted is that of the mackerel and the mugil, although they never exceed a foot in length. The Greeks of Taurida, salt them in great quantities. When thus kept for a year they are much esteemed, but when used earlier they are hard.

The name of the Mackerel (*Macarellus*) appears in Albertus Magnus, and in Arnaud de Villeneuve. The etymology of the word is not agreed on. Some derive it from *macularius* or *maculariolus* on account of its spots; others from *μακαριος*, on account of its excellence; but there is little probability that a word, used time immemorially by the northern nations, should come from the languages of the south, in as much as there are many sea ports on the southern coasts where this term is not known.

The Languedociens, the Provençals, the Niçards and the Genoese, call this fish *auriol*, *auriou*, *aurneau*, which Rondeletius explains by *peis d'aurioul* (fish of April). According to Salvian *macarello* is the name at Rome; the Venetians, says the same author, call it *scombro*, the Neapolitans *lacerto*, the Spaniards *cavallo*;—denominations confirmed by more modern writers. The Sardinians name it *pisaro*. In Sicily, according to M. Rafinesque, the derivatives of Scomber are only employed: *scarmu* or *scombru* at Palermo, *strumbu* at Messina, *scrumiu* at Catania, *sgambirri* at Syracuse, &c. The Greeks and Russians of the Taurida call it *scumbro*; but according to Forskal, the Greeks at Constantinople name it *κολος*, and the Turks *kolios-baluk*; it is also denominated *scombri*.

Among the fishes which the ancients were accustomed to salt, were small species which bore the particular names of *scomber*, *colias* and *cordyla*, and which were comprised under the generic name of *lacertus*. There is reason to believe that it was the common mackerel and the neighbouring species, of which we are afterwards to speak, which were meant by this term. What is said of them by Pliny proves them to have been com-

mon and of small size. They were rolled up in paper, and, according to Martial, the verses of bad poets were threatened to be applied to this use, as in modern times unsaleable verses may serve for wrapping up pepper or cinnamon.

The scomber is the fish most frequently mentioned by ancient authors. Aristotle classes it among the fishes which congregate in shoals, and which periodically visit Pont Euxine. He associates it with the tunny, the pelamides and colias, but he says it is inferior to them in strength. Seen in the water it appeared of the colour of sulphur. It was fished in great quantities on the coasts of *Boetica* and *Mauritania*, where it arrived by the Pillars of Hercules. It made up on these coasts for the want of the tunny, when this fish did not appear. An island in the neighbourhood of Carthagera, and which covers the entrance of the bay of this town, was called *Scombraria*, from the abundance of these fishes. This name passed afterwards to the cape which is to the east of Carthagera, and which is now called *Capo-di-Palos*. These different indications may be referred to the mackerel with the greater probability, when it is added that it bears to this day its ancient name but little altered in certain districts of Italy and Greece.

As to the name *Colias*, it seems to have denoted sometimes one species, sometimes another. On the one hand, Pliny tells us that it was the length of the genus *Lacertus*; while on the other, Hicesius in Athenæus makes it larger than the scomber. According to Martial, it was less esteemed as food, being considered more glutinous and acid. It was fished and cured to a great extent at Parium on the Hellespont, in one of the towns called Amylea, but above all at Sex on the coast of Boetica, (now Almunecar) a place celebrated for all sorts of cured fish.

This *Colias* may then be one of the species congenerous with mackerel which is found in the Mediterranean, and which we shall afterwards describe; either the *coigniol* of the Marseillois, of which the name seems to preserve a trace of this etymology, or the *pneumatophorus* or *lacerto* of Sardinia, which has preserved only the generic term. These two species, not less abundant than the mackerel, are inferior in size and taste.

As to the *Cordyla*, in particular, it is known through Pliny

that it was properly a small pelamide, and consequently a synonym of *σχορδύλος* of Aristotle,—that is to say, a pelamide or a young tunny, such for example as came from Palus Mæotis. But there are young tunnys on the coast of Italy as well as in the sea of Azof; and there is nothing besides to prove that this term has not changed its original meaning, and been applied to some species constantly of small size.

This is the place to say a few words of the *Garum*,—a preparation so celebrated among the gourmands of Ancient Rome, and which was made chiefly with the intestines and blood of the scomber. According to Pliny, it was an invention of the Greeks, who prepared it with a fish to which they gave the name of *garon*. In point of fact this name is found in a verse of Sophocles, cited by Julius Pollux.

Different receipts have been preserved for preparing this celebrated sauce. According to one, the intestines of fishes, such as atherines, anchovies, mullets, &c. were partially salted, then put into a vessel and exposed to the sun, turning the mass over many times to excite decomposition to a certain extent. When the favourable moment arrived, a kind of close-worked basket was inserted. The liquid portion of the mixture which percolated through the meshes of the basket was the *garum*,—that which remained on account of its firmer consistence, bore the name of *alec*.

In Bithynia a different process was followed. The fishes were put into a vessel with flour, adding to each modium two measures of salt. After remaining thus for a night, the mixture was placed in an open earthen vessel, which was exposed to the sun for two or three months, stirring it carefully. It was then covered up. Some poured above the mixture a double quantity of old wine. There was also a way of preparing this *garum* much sooner by artificial heat, or cooking, in place of exposing it to the sun. For this purpose a pickle was made strong enough to carry an egg; the fish was put in with a little marjoram, and after boiling and cooling, the liquid was passed through a strainer till clear.

Finally, a better *garum* than these was made by enclosing in a vessel the intestines and blood of the tunny with salt, and leaving the mixture for nearly two months, after which the

vessel was pierced. The liquid which flowed out was the sanguinolent *garum*, (*αιμαριον*.)

We can scarcely conceive how operations so disgusting could produce a substance agreeable to the taste ; but the unanimous testimony of the ancients does not permit us to doubt either of their nature or result. “ Aliud etiamnum liquoris exquisiti genus, (says Pliny,) quod garon vocavere, intestinis piscium cæterisque quæ abjicienda essent, sale maceratis, ut sit illa putrescentium sanies.” Apparently this *garum*, similar to those half-putrid, half-salt fluids which run from certain cheeses, had the faculty of awakening the appetite, and exciting digestion ; it appears to have been a very bitter substance. Seneca speaks of it as one of the causes which most injured the health of the rich in his time : “ Pretiosam malorum piscium saniem, non credis urere salsa tabe præcordia ? quid ? illa purulenta et quæ tantum non ab ipso igne in os transferantur judicas in ipsis visceribus extingui.”—Its odour, according to Martial, was detestable. But notwithstanding all this, the *garum* was a sauce much sought after, and very dear. It was used as a sauce for oysters ; and Apicius thought of drowning mullets in it to eat them in full perfection.

An esteemed *garum* was made at Clazomene, at Pompeia, and at Leptis, but the most celebrated was that of Carthage. It was made from the scombers which approached the shores of Boetica and Mauritania, which were fished for this purpose, and was named *garum sociorum*, a designation of which the reason is not well known. With the exception of certain perfumes, it was the dearest of all liquors. There was also made at Antibes, with the intestines of the tunny, another kind of *garum* called *muria*, much inferior, however, to that of the scomber.

Rondeletius speaks of a kind of *garum* which was prepared in his time, by dropping picarels (*Smaris*, Lin.) into brine, and which he had tasted in the house of the celebrated Bishop of Montpellier, William Pelicier ; but I do not find it mentioned by modern authors, or that the use of it had been continued. Belon relates also, that in his time the *garum* was in as great repute as ever, and that there was not a fish-shop in Constantinople where it was not sold. It was made from the

intestines of the mackerel and saurel ; but I have not remarked that recent travellers have spoken of it.—*Hist. des Poissons*, vol. viii.

ART. X.—*Note respecting the Great Scientific Meeting at York.*

By GEORGE HARVEY, Esq. F. R. SS. L. and E., Member of the British Association for the Promotion of Science, Honorary Member of the Yorkshire Philosophical Society, &c. &c. Communicated by the Author.

IN all that has been written respecting the migratory scientific meetings that have in latter years taken place on the continent, or the great meeting lately held for the same purposes at York, I do not remember any allusion having been made to the circumstance, that those meetings fulfil, in a very remarkable manner, one of the splendid predictions of Bacon.

As the fixed and stationary societies of Europe grew out of his beautiful description of “Solomon’s House,” so has the irresistible tide of events at length brought about those “*circuits or visits of divers principal cities of the kingdom*,” which he regarded as another distinguishing feature of the “New Atlantis,” and which it has been reserved for our own day to see so completely fulfilled.

In the establishment of these “*circuits*,” it is evident that the views of Bacon must have been entirely prospective ; since societies having fixed and stationary places of meeting must have first been permitted to exist before any attempt could be made to form associations of a migratory kind. Such stationary meetings have very long existed ; but they stand as great insulated points in the hemisphere of knowledge, and are known only to each other by the interchange of their transactions, or the occasional diplomas sent from one to the other. Poisson and Arago at Paris, Berzelius at Stockholm, Gauss at Göttingen, Oersted at Copenhagen, and Encke at Berlin, have long cultivated the sciences with success in their respective academies ; but a new bond of sympathy was wanting to knit these illustrious men in closer fellowship together. The “*circuits*” of Bacon’s Atlantis will do this ; and the “*visits*” at Berlin

and York cannot but infuse fresh vigour into the world of science.

Those who live far removed from the intercourse of scientific men can best estimate the advantages resulting from personal communion. Many a solitary and dejected heart has been quickened and revived by oral communications with illustrious men; and we cannot but hope, that while the fixed academies have in general so abundantly fulfilled the magnificent conceptions of Bacon, that these "circuits of divers principal cities" will equally realize all that his prophetic mind anticipated.

PLYMOUTH, *February 16, 1832.*

ART. XI.—*On the Charter lately granted to the Astronomical Society of London.* By a CORRESPONDENT.

IT may be as well to put on record in the pages of the *Edinburgh Journal of Science*, now that the public mind has been awakened to the consideration of some of the many impediments to which the free course of science is subjected in this country, the various *items* of the large sum lately paid by the Astronomical Society of London for obtaining a charter. That a charter is advantageous and most honourable will be instantly admitted by all, and by no one more readily than the astronomer, who, having constantly before him the most splendid and magnificent examples of all that is glorious and high, cannot but receive with satisfaction and pride, from the pure fountain of honour in his own beloved country, an instrument which gives to a society to which he is ardently attached privileges and powers which it did not before possess. The astronomer, however, while he deeply venerates the source from which this honourable distinction is derived, cannot but feel, that an institution devoted to the sublime and lofty purposes of the heavens, is subjected to precisely the same expences, and the same rigid and tiresome official forms, as a company whose sole object in obtaining a charter is for purposes of gain, and which, instead of adding to the intellectual renown of the country and the age, may cause one name more to be inscribed on the list of

visionary speculations which has given so remarkable and melancholy a feature to our own times.

That a variety of offices should exist through which all the king's grants should pass, and that the same should be narrowly inspected by his officers to prevent fraud or imposition, will at once be admitted, since, among the boundless artifices employed in the world, his Majesty might otherwise have his royal favour abused. But in subjects purely scientific, and in a society in particular, devoted to such noble and lofty objects as the astronomical, it may be permitted us to wish that some relaxation of the severe and ordinary fees might be allowed. The circumstances of this charter undeniably prove, that a learned body is favoured by no advantages from the State on the ground of *SCIENCE*;—that the granting of the charter is looked upon, at least by the public offices through which it has to pass, as a thing purely commercial;—and that it is even charged for "*extra dispatch*," just as a company formed to work the mines of Peru may be eager to send forth its engineers to be the first in the field of gold.

CASH PAID, AND TO WHOM.

<i>Hearing.</i> —Mr Att.-Gen. and Clerk,	L.6	2	6	
Mr Sol.-Gen. and Clerk,	6	2	6	
<i>Report.</i> — Mr Att.-Gen. and Clerk,	12	12	0	
Mr Sol.-Gen. and Clerk,	11	11	0	
				L.36 8 0
<i>Perusal.</i> —At the above, with Bill, &c.	14	10	0	
Paid for Warrant,	-	1	10 0	
Engrossing Clerk,	-	1	1 0	
Transcript, Messenger, &c.	4	7	6	
				21 8 6
<i>Signet Office.</i> —Fees,	-	30	10 0	
Gratuity,	-	3	3 0	
Rec. &c.	-	1	1 0	
Extra dispatch,	-	1	11 6	
				36 5 6
<i>Privy Seal Office.</i> —Fees and Stamp,	30	5	0	
Gratuity,	-	3	3 0	
Record Keeper,	1	1	0	

	Private Seal,	L.2	0	0	
	Extra dispatch,	1	11	6	
					L.38 0 6
<i>Patent Office.</i>	Office,	-	-	5 17 8	
	Stamps,	-	-	30 2 0	
	Boxes,	-	-	1 11 6	
	Deputy,	-	-	2 2 0	
	Hanaper,	-	-	14 13 6	
	Deputy,	-	-	1 1 0	
	Receipt,	-	-	5 5 0	
	Gentlemen of the Cham-				
	bers,	-	-	5 5 0	
	Private Seal,	-	-	2 2 0	
	Sealers,	-	-	1 1 0	
	Copy for Lord Chancel-				
	lor,	-	-	0 10 6	
	Lord Chancellor's Por-				
	ter,	-	-	0 10 6	
	Clerk,	-	-	1 1 0	
					71 2 8
<i>Sec. of State's Office.</i>	Passing Charter,	41	4	6	
	Extra dispatch,	5	5	0	
					46 9 6
	Total paid to Mr Plasket,	L.249	14	8	
Cost of Vellum, Paper, Engrossing, &c. Messen-					
gers, &c. paid by Mr Hoppe, (Note A,)	-	12	12	0	
Case for Counsel, relative to the Charter,	-	3	5	6	
Total paid by Mr Hoppe,	L.265	12	2		

Note A.—It is pleasing to notice the great liberality of Mr Hoppe. That gentleman, at the special request of Mr Stratford, the late secretary of the society, tendered his services *gratuitously*. It is hoped, that, as a professional man, he will be amply rewarded.

ART. XII.—*An Estimate of the Philosophical Character of Dr Priestley.* By WILLIAM HENRY, M. D., F. R. S., &c. &c. (From the Report of the Meeting of the British Association.)

THE principal source of the materials of the following pages, is the work, in which the discoveries of Dr Priestley were originally announced to the public. It consists of six volumes in octavo, which were published by him, at intervals between the years 1774 and 1786; the first three under the title of "*Experiments and Observations on different kinds of Air;*" and the last three under that of "*Experiments and Observations relating to various Branches of Natural Philosophy, with a continuation of the Observations on Air.*" These volumes were afterwards methodized by himself, and compressed into three octavos, which were printed in 1790. As a record of facts, and as a book of reference, the systematized work is to be preferred. But as affording materials for the history of that department of science, which Dr Priestley cultivated with such extraordinary success; and, still more, for estimating the value of his discoveries, and adjusting his station as an experimental philosopher, the simple narrative, which he originally gave in the order of time, supplies the amplest and the firmest ground-work.

In every thing that respects the history of this branch of experimental philosophy, the writings and researches of Dr Priestley, to which I have alluded, are peculiarly instructive. They are distinguished by great merits, and by great defects; the latter of which are wholly undisguised by their author. He unveils, with perfect frankness, the whole process of reasoning, which led to his discoveries; he pretends to no more sagacity than belonged to him, and sometimes disclaims even that to which he was fairly entitled; he freely acknowledges his mistakes, and candidly confesses when his success was the result of accident, rather than that of judicious anticipation; and by writing historically and analytically, he exhibits the progressive improvement of his views, from their first dawnings, to their final and distinct developement. Now, with whatever delight we may contemplate a systematic arrangement,

the materials of which have been judiciously selected, and from which every thing has been excluded, that is not essential to the harmony of the general design, yet there can be no question that, as elucidating the operations of the human mind, and enabling us to trace and appreciate its powers of invention and discovery, the analytic method of writing has decided advantages.

To estimate, justly, the extent of Dr Priestley's claim to philosophical reputation, it is necessary to take into account the state of our knowledge of gaseous chemistry, at the time when he began his inquiries. Without underrating what had been already done by Van Helmont, Ray, Hooke, Mayow, Boyle, Hales, Macbride, Black, Cavendish, and some others, Priestley may be safely affirmed to have entered upon a field, which, though not altogether untilled, had yet been very imperfectly prepared to yield the rich harvest, which he afterwards gathered from it. The very implements, with which he was to work, were for the most part to be invented; and of the merits of those, which he did invent, it is a sufficient proof that they continue in use to this day, with no very important modifications. All his contrivances for collecting, transferring, and preserving different kinds of air, and for submitting those airs to the action of solid and liquid substances, were exceedingly simple, beautiful, and effectual. They were chiefly, too, the work of his own hands, or were constructed under his directions by unskilled persons; for the class of ingenious artists, from whom the chemical philosopher now derives such valuable aid, had not then been called into existence by the demands of the science. With a very limited knowledge of the general principles of chemistry, and almost without practice in its most common manipulations;—restricted by a narrow income, and at first with little pecuniary assistance from others;—compelled, too, to devote a large portion of his time to other pressing occupations, he nevertheless surmounted all obstacles; and in the career of discovery, outstripped many who had long been exclusively devoted to science, and were richly provided with all appliances and means for its advancement.

It is well known that the accident of living near a public brewery at Leeds, first directed the attention of Dr Priestley to

pneumatic chemistry, by casually presenting to his observation the appearances attending the extinction of lighted chips of wood, in the gas which floats over fermenting liquors. He remarked, that the smoke formed distinct clouds floating on the surface of the atmosphere of the vessel, and that this mixture of air and smoke, when thrown over the sides of the vat, fell to the ground; from whence he deduced the greater weight of this sort of air than of atmospheric air. He next found that water imbibes the new air, and again abandons it when boiled or frozen. These more obvious properties of fixed air having been ascertained, he extended his inquiries to its other qualities and relations; and was afterwards led by analogy to the discovery of various other gases, and to the investigation of their characteristic properties.

It would be inconsistent with the scope of this Essay to give a full catalogue of Dr Priestley's discoveries, or to enumerate more of them than are necessary to a just estimate of his philosophical habits and character. He was the unquestionable author of our first knowledge of oxygen gas, of nitrous oxide, of muriatic, sulphurous, and fluor acid gases, of ammoniacal gas, and of its condensation into a solid form by the acid gases. Hydrogen gas was known before his time; but he greatly extended our acquaintance with its properties. Nitrous gas, barely discovered by Dr Hales, was first investigated by Priestley, and applied by him to eudiometry. To the chemical history of the acids derived from nitre, he contributed a vast accession of original and most valuable facts. He seems to have been quite aware that those acids are essentially gaseous substances, and that they might be exhibited as such, provided a fluid could be found that is incapable of absorbing or acting upon them. * He obtained, and distinctly described, † the curious crystalline compound of sulphuric acid with the vapour of nitrous acid, or, more correctly, of sulphuric and hyponitrous acids, which, being of rare occurrence, was forgotten, and has since been rediscovered, like many other neglected anticipations of the same author. He greatly enlarged our knowledge of the important class of metals, and traced out many of their most interesting relations to oxygen and to acids. He unfold-

* Series I. vol. ii. p. 175.

† Series II. vol. i. p. 26.

ed, and illustrated by simple and beautiful experiments, distinct views of combustion ; of the respiration of animals, both of the inferior and higher classes ; of the changes produced in organized bodies by putrefaction, and of the causes, that accelerate or retard that process ; of the importance of azote as the characteristic ingredient of animal substances, obtainable by the action of dilute nitric acid on muscle and tendon ; of the functions and economy of living vegetables ; and of the relations and subserviency which exist between the animal and vegetable kingdoms. After trying, without effect, a variety of methods, by which he expected to purify air vitiated by the breathing of animals, he discovered that its purity was restored by the growth of living and healthy vegetables, freely exposed to the solar light.

It is impossible to account for these, and a variety of other discoveries, of less importance singly, but forming altogether a tribute to science, greatly exceeding, in richness and extent, that of any contemporary, without pronouncing that their author must have been furnished by nature with intellectual powers, far surpassing the common average of human endowments. If we examine, with which of its various faculties the mind of Dr Priestley was most eminently gifted, it will, I believe, be found that it was most remarkable for clearness and quickness of apprehension, and for rapidity and extent of association. On these qualities were founded that apparently intuitive perception of analogies, and that happy facility of tracing and pursuing them through all their consequences, which led to several of his most brilliant discoveries. Of these analogies many were just and legitimate, and have stood the test of examination by the clearer light, since reflected upon them from the improved condition of science. But, in other cases, his analogies were fanciful and unfounded, and led him far astray from the path, which might have conducted him directly to truth. It is curious, however, as he himself observes, that in missing one thing, of which he was in search, he often found another of greater value. In such cases, his vigilance seldom failed to put him in full possession of the treasure upon which he had stumbled. Finding by experience, how much chance had to do with the success of his investigations, he re-

solved to multiply experiments, with the view of increasing the numerical probabilities of discovery. We find him confessing, on one occasion, that he "was led on, by a random expectation of some change or other taking place." In other instances, he was influenced by theoretical views of so flimsy a texture, that they were dispersed by the first appeal to experiment. "These mistakes," he observes, "it was in my power to have concealed; but I was determined to show how little mystery there is in the business of experimental philosophy; and with how little sagacity, discoveries, which some persons are pleased to consider great and wonderful, have been made." Candid acknowledgments of this kind were, however, turned against him by persons envious of his growing fame; and it was asserted that *all* his discoveries, when not the fruits of plagiarism, were "lucky guesses," or owing to mere chance. Such detractors, however, could not have been aware of the great amount of credit, that is due to the philosopher, who at once perceives the value of a casual observation, or of an unexpected result; who discriminates what facts are trivial, and what are important; and selects the latter, to guide him through difficult and perplexed mazes of investigation. In the words of D'Alembert, "*Ces hazards ne sont que pour ceux qui jouent bien.*"

The talents and qualifications, which are here represented as having characterized the mind of Dr Priestley, though not of the rarest kind, or of the highest dignity, were yet such, as admirably adapted him for improving chemical science, at the time when he lived. What was then wanted, was a wider field of observation;—an enlarged sphere of chemical phenomena;—an acquaintance with a far greater number of individual bodies, than were then known; from the properties of which, and from those of their combinations, tentative approximations to general principles might at first be deduced; to be confirmed or corrected, enlarged or circumscribed, by future experience. It would have retarded the progress of science, and put off, to a far distant day, that affluence of new facts, which Priestley so rapidly accumulated, if he had stopped to investigate, with painful and rigid precision, all the minute circumstances of temperature, of specific gravity, of absolute

and relative weights, and of crystalline structure, on which the more exact science of our own times is firmly based, and from which its evidences must henceforward be derived. Nor could such refined investigations have then been carried on with any success, on account of the imperfection of philosophical instruments. It would have been fruitless, also, at that time, to have indulged in speculations respecting the ultimate constitution of bodies; speculations that have no solid groundwork, except in a class of facts developed within the last thirty-five years, all tending to establish the laws of combination in definite and in multiple proportions, and to support the still more extensive generalization, which has been reared by the genius of Dalton.

It was, indeed, by the activity of his intellectual faculties, rather than by their reach or vigour, that Dr Priestley was enabled to render such important services to natural science. We should look, in vain, in any thing that he has achieved, for demonstrations of that powerful and sustained attention, which enables the mind to institute close and accurate comparisons;—to trace resemblances that are far from obvious;—and to discriminate differences that are recondite and obscure. The analogies, which caught his observation, lay near the surface, and were eagerly and hastily pursued; often, indeed, beyond the boundaries, within which they ought to have been circumscribed. Quick as his mind was in the perception of resemblances, it appears (probably for that reason) to have been little adapted for those profound and cautious abstractions, which supply the only solid foundations of general laws. In sober, patient, and successful induction, Priestley must yield the palm to many others, who, though far less fertile than himself in new and happy combinations of thought, surpassed him in the use of a searching and rigorous logic; in the art of advancing, by secure steps, from phenomena to general conclusions;—and again in the employment of general axioms as the instruments of farther discoveries.

Among the defects of his philosophical habits, may be remarked, that he frequently pursued an object of inquiry too exclusively, neglecting others, which were necessarily connected with it, and which, if investigated, would have thrown great

light on the main research. As an instance, may be mentioned his omitting to examine the relation of gases to water. This relation, of which he had indistinct glimpses, was a source of perpetual embarrassment to him, and led him to imagine changes in the intimate constitution of gases, which were in fact due to nothing more than an interchange of place between the gas in the water and that above the water, or between the former and the external atmosphere. Thus he erroneously supposed that hydrogen gas was transmuted into azotic gas, by remaining long confined by the water of a pneumatic cistern. The same eager direction of his mind to a single object, caused him, also, to overlook several new substances, which he must necessarily have obtained, and which, by a more watchful care, he might have secured and identified. At a very early period of his inquiries, (viz. before November 1771), he was in possession of oxygen gas from saltpetre, and had remarked its striking effect on the flame of a candle; but he pursued the subject no farther until August 1774, when he again procured the same kind of gas from the red oxide of mercury, and, in a less pure state, from red lead. Placed thus a second time within his grasp, he did not omit to make prize of this, his greatest, discovery. He must, also, have obtained chlorine by the solution of manganese in spirit of salt; but it escaped his notice, because, being received over mercury, the gas was instantly absorbed. If he had employed a bladder, as Scheele afterwards did, to collect the product of the same materials, he could not have failed to anticipate the Swedish philosopher, in a discovery not less important than that of oxygen gas. Carbonic oxide early and repeatedly presented itself to his observation, without his being aware of its true distinctions from other kinds of inflammable air; and it was reserved for Mr Cruickshank of Woolwich to unfold its real nature and characters. It is remarkable, also, that in various parts of his work, Dr Priestley has stated facts, that might have given him a hint of the law, since unfolded by the sagacity of M. Gay-Lussac, "that gaseous substances combine in definite volumes." He shows that

1 measure of fixed air unites with 1 $\frac{1}{2}$ measure of alkaline air,

1 measure of sulphurous acid with 2 measures of do.

1 measure of fluor acid with 2 measures of do.

1 measure of oxygen gas with 2 measures nitrous, very nearly ;
and that by the decomposition of 1 vol. of ammonia, 3 vols. of hydrogen are evolved.

Let not, however, failures such as these, to reap all that was within his compass, derogate more than their due share from the merits of Dr Priestley ; for they may be traced to that very ardour of temperament, which, though to a certain degree a disqualification for close and correct observation, was the vital and sustaining principle of his zealous devotion to the pursuit of scientific truth. Let it be remembered, that philosophers of the loftiest pretensions are chargeable with similar oversights ;—that even Kepler and Newton overlooked discoveries, upon the very confines of which they trod, but which they left to confer glory on the names of less illustrious followers.

Of the general correctness of Dr Priestley's experiments it is but justice to him to speak with decided approbation. In some instances, it must be acknowledged, that his results have been rectified by subsequent inquirers, chiefly as respects quantities and proportions. But of the immense number of new facts originating with him, it is surprising how very few are at variance with recent and correct observations. Even in these few examples, his errors may be traced to causes connected with the actual condition of science at the time ; sometimes to the use of impure substances, or to the imperfection of his instruments of research ; but never to carelessness of inquiry or negligence of truth. Nor was he more remarkable for the zeal, with which he sought satisfactory evidence, than for the fidelity, with which he reported it. In no one instance is he chargeable with mis-stating, or even with straining or colouring, a fact, to suit an hypothesis. And though this praise may, doubtless, be conceded to the great majority of experimental philosophers, yet, Dr Priestley was singularly exempt from that disposition to view phenomena through a coloured medium, which sometimes steals imperceptibly over minds of the greatest general probity. This security he owed to his freedom from all undue attachment to hypotheses, and to the facility, with which he was accustomed to frame and abandon

them ;—a facility resulting not from habit only, but from principle. “Hypotheses” he pronounces, in one place, “to be a cheap commodity ;” in another to be “of no value except as the parents of facts ;” and so far as he was himself concerned, he exhorts his readers “to consider new facts only as discoveries, and to draw conclusions for themselves.” The only exception to this general praise is to be found in the pertinacity with which he adhered, to the last, to the Stahlian hypothesis of phlogiston ; and in the anxiety which he evinced, to reconcile to it new phenomena, which were considered by almost all other philosophers, as proofs of its utter unsoundness. But this anxiety, it must be remembered, was chiefly apparent at a period of life, when most men feel a reluctance to change the principle of arrangement, by which they have been long accustomed to class the multifarious particulars of their knowledge.

In all those feelings and habits that connect the purest morals with the highest philosophy, (and that there is such a connection no one can doubt,) Dr Priestley is entitled to unqualified esteem and admiration. Attached to science by the most generous motives, he pursued it with an entire disregard to his own peculiar interests. He neither sought, nor accepted when offered, any pecuniary aid in his philosophical pursuits, that did not leave him in possession of the most complete independence of thought and of action. Free from all little jealousies of contemporaries or rivals, he earnestly invited other labourers into the field, which he was cultivating ; gave publicity, in his own volumes, to their experiments ; and, with true candour, was as ready to record the evidence which contradicted, as that which confirmed, his own views and results. Every hint, which he had derived from the writings or conversation of others, was unreservedly acknowledged. As the best way of accelerating the progress of science, he recommended and practised the early publication of all discoveries ; though quite aware that, in his own case, more durable fame would often have resulted from a delayed and more finished performance. “Those persons,” he remarks, “are very properly disappointed, who, for the sake of a little more reputation,

delay publishing their discoveries, till they are anticipated by others."

In perfect consistency with that liberality of temper, which has been ascribed to Dr Priestley, it may be remarked also, that he took the most enlarged views of the scope and objects of Natural Science. In various passages of his works he has enforced with warm and impressive eloquence, the considerations, that flow from the contemplation of those arrangements in the natural world, which are not only perfect in themselves, but are essential parts of one grand and harmonious design. He strenuously recommends experimental philosophy as an agreeable relief from employments, that excite the feelings or overstrain the attention; and he proposes it to the young, the high-born, and the affluent, as a source of pleasure unalloyed with the anxieties and agitations of public life. He regarded the benefits of its investigations, not merely as issuing in the acquirement of new facts, however striking and valuable; nor yet in the deduction of general principles, however sound and important; but as having a necessary tendency to increase the intellectual power and energy of man, and to exalt human nature to the highest dignity, of which it is susceptible. The springs of such inquiries he represents as inexhaustible; and the prospects, that may be gained by successive advances in knowledge, as in themselves "truly sublime and glorious."

Into our estimate of the intellectual character of an individual, the extent and the comprehensiveness of his studies must always enter as an essential element. Of Dr Priestley it may be justly affirmed, that few men have taken a wider range over the vast and diversified field of human knowledge. In devoting, through the greater part of his life, a large portion of his attention to theological pursuits, he fulfilled, what he strongly felt to be his primary duty as a minister of religion. This is not the fit occasion to pronounce an opinion of the fruits of those inquiries, related as they are to topics, which still continue to be agitated as matters of earnest controversy. In Ethics, in Metaphysics, in the philosophy of Language, and in that of General History, he expatiated largely. He has given particular histories of the sciences of electricity and of optics, characterized by strict impartiality, and by great per-

spicuity of language and arrangement. Of the mathematics, he appears to have had only a general or elementary knowledge; nor, perhaps, did the original qualities, or acquired habits, of his mind fit him to excel in the exact sciences. On the whole, though Dr Priestley may have been surpassed by many, in vigour of understanding and capacity for profound research, yet it would be difficult to produce an instance of a writer more eminent for the variety and versatility of his talents, or more meritorious for their zealous, unwearied, and productive employment.

APPENDIX.

Since the foregoing pages were written, I have added a few remarks on a passage contained in a recent work of Victor Cousin, in which that writer has committed a material error as to the origin of Dr Priestley's philosophical discoveries. "La chimie," he observes, "est une création du dixhuitième siècle, une création de la France; c'est l'Europe entière qui a appelé chimie Française le mouvement qui a imprimé à cette belle science une impulsion si forte et une direction si sage; c'est à l'exemple et sur les traces de Lavoisier, de Guyton, de Fourcroy, de Berthollet, de Vauquelin, que se sont formés et que marchent encore les grands chimistes étrangers, ici Priestley et Davy; là Klaproth et Berzelius." (*Cours de l'Histoire de la Philosophie*, tom. i. p. 25.)

It is to be lamented that so enlightened a writer as Victor Cousin, yielding, in this instance, to the seduction of national vanity, should have advanced pretensions in behalf of his countrymen, which have no foundation in truth or justice. Nothing can be more absurd or unprofitable than to claim honours in science, either for individuals or for nations, the title to which may be at once set aside by an appeal to public and authentic records.

It was in England, not in France, that the first decided advances were made in our knowledge of elastic fluids. To say nothing of anterior writers, Dr Black had traced the causticity acquired by alkalies, and by certain earths, to their being freed from combination with fixed air; and Mr Cavendish, in 1766, had enlarged our knowledge of that gas and of inflammable

air. In England, the value of these discoveries was fully appreciated; in France, little or no attention was paid to them, till the philosophers of that country were roused by the striking phenomena exhibited by the experiments of Priestley. Lavoisier, it is true, had been led, by an examination of evidence derived from previous writers, to discard the hypothesis of phlogiston. The discovery of oxygen gas by Dr Priestley not only completed the demonstration of its fallacy, but served as the corner-stone of a more sound and consistent theory. By a series of researches executed at great expence, and with consummate skill, the French philosopher verified in some cases, and corrected in others, the results of his predecessors, and added new and important observations of his own. Upon these united, he founded that beautiful system of general laws, chiefly relating to the absorption of oxygen by combustible bodies, and to the constitution of acids, to which, alone, the epithet of the Antiphlogistic or French theory of chemistry is properly applied. Of the genius manifested in the construction of that system, and the taste apparent in its exposition, it is scarcely possible to speak with too much praise. But it is inverting the order of time to assert, that it had any share in giving origin to the researches of Priestley, which were not only anterior to the French theory, but were carried on under the influence of precisely opposite views. This, too, may be asserted of the discoveries of Scheele, who, at the same period with Dr Priestley, was following, in a distant part of Europe, a scarcely less illustrious career.

It is the natural progress of most generalizations in science, that at first too hasty and comprehensive, they require to be narrowed as new facts arise. This has happened to the theory of Lavoisier, in consequence of its having been discovered that combustion is not necessarily accompanied with an absorption of oxygen, and that acids exist independently of oxygen, regarded by him as the general acidifying principle. But after all the deductions, that can justly be made on that account from the merits of Lavoisier, he must still hold one of the highest places among those illustrious men, who have advanced chemistry to its present rank among the physical sciences. It is deeply to be lamented that his fame, otherwise unsullied,

should have been stained by his want of candour and justice to Dr Priestley, in appropriating to himself the discovery of oxygen gas. This charge, often preferred and never answered, would not have been revived in this place, but for the claim so recently and indiscreetly advanced by M. Victor Cousin. To the credit of Dr Priestley it may be observed, that in asserting his own right, he exercised more forbearance, than could reasonably have been expected under such circumstances. In an unpublished letter to a friend, he thus alludes to the subject of M. Lavoisier's plagiarism. "He," (M. Lavoisier) "is an *Intendant of the Finances*, and has much public business, but finds leisure for various philosophical pursuits, for which he is exceedingly well qualified. He ought to have acknowledged that my giving him an account of the air I had got from *Mercurius Calcinatus*, and buying a quantity of M. Cadet while I was at Paris, led him to try what air it yielded, which he did presently after I left. I have, however, barely hinted at this in my second volume." The communication alluded to was made by Dr Priestley to M. Lavoisier in October 1774; and the Memoir, in which the latter assumes to himself the discovery that *mercurius calcinatus* (red oxide of mercury) affords oxygen gas when distilled *per se*, was not read to the Academy of Sciences before April 1775. In evincing so little irritability about his own claim, and leaving its vindication with calm and just confidence to posterity, the English philosopher has lost nothing of the honour of that discovery, which is now awarded to him, by men of science of every country, as solely and undividedly his own.

ART. XIII.—*On the Chlorides of Sulphur, Selenium, and Tellurium.* By HEINRICH ROSE, Professor of Chemistry in the University of Berlin.

Chloride of Sulphur.

M. H. ROSE prepares pure chloride of sulphur by passing dry chlorine gas over flowers of sulphur till they are nearly all dissolved, then distilling by a very gentle heat. Prepared in this way it contains no excess either of chlorine or sulphur.

He analyzed it by decomposing the chloride with fuming nitric acid in a close bottle, and precipitating the sulphuric acid by chloride of barium.

The mean of two analyses gave

	Theory.	Experiment.
Sulphur 2 atoms = 4.022	= 52.39	= 52.54
Chlorine 1 atom, = 4.426	= 47.61	= 47.46
8.448	100	100 *

agreeing with the previous results of Thomson and Bucholz.

In systems of chemistry another chloride is generally described, on the authority of Davy and Dumas, containing twice as much chlorine, and it is said to be formed by passing chlorine gas through the above chloride as long as any is absorbed. M. Rose tried in vain to obtain by this process any definite compound; nor was he more successful in obtaining an atomic compound containing more sulphur by digesting the chloride on flowers of sulphur at different temperatures, and analyzing the solutions obtained. He therefore concludes that we are at present acquainted with no definite compound of these two elements but the di-chloride above described, and which was discovered by Dr Thomson in 1803.

Di-chloride of Selenium.

The liquid chloride of selenium has much resemblance to the chloride of sulphur. Both are oily fluids, have a peculiar though dissimilar colour, and when decomposed by water, form oxygen acids of similar composition, while sulphur or selenium is precipitated. According to Berzelius the fluid chloride of selenium has also the same composition, or it is a di-chloride.†

Chlorides of Tellurium.

Bi-chloride.—The telluret of silver brought from the Altai

* *Pogg. Ann.* xxi. p. 433. Rose considers this chloride as a compound of atom to atom, for with Berzelius he makes the atom of chlorine 221.325.

† Dr Thomson, (*Inorganic Chem.* i. p. 292,) makes it a neutral chloride, consisting of 4.5 chlorine, + 5 selenium, though Berzelius, from whose experiments he deduces the composition, assigns to it double the quantity of selenium.

mountains by his brother, has supplied M. H. Rose with the means of examining the chlorides of tellurium, of which we hitherto knew so little.

When metallic tellurium is very gently heated in a stream of chlorine gas a white crystalline chloride is formed, which may be distilled over. It has much resemblance to the solid chloride of selenium. At common temperatures it remains solid, but by a gentle heat melts into a brown liquid, which on cooling solidifies and again becomes white. At higher temperatures it sublimes. It attracts moisture, but does not volatilize when exposed to the air. Water decomposes it, forming a milky liquid containing oxide of tellurium and muriatic acid, but not in sufficient quantity to dissolve the oxide.

Dissolved in water, acidulated by sulphuric acid, and precipitated by nitrate of silver, it was found to be composed of

			Theory.		Experiment.
Chlorine	2 atoms,	8.853	=	52.33	— 52.13
Tellurium	1 atom,	8.064	=	47.67	— 47.87
		16.917		100	100

It is therefore like the solid chloride of selenium, which in appearance it resembles so much, a *bi*-chloride.

Chloride.—When tellurium is strongly heated in chlorine gas, a black fluid distils over, and condenses into a solid black substance. The vapour is of a violet colour, resembling that of iodine, but paler. It deliquesces in the air. With water it forms a greyish-black solution, being decomposed into metallic tellurium, oxide of tellurium, and muriatic acid, which dissolves a portion of the oxide. If water acidulated with sulphuric acid be employed, the whole of the oxide is dissolved, and the metallic tellurium remains in small crystalline fibres. The weight of the metal thus obtained = 32.04 per cent. of the chloride employed, being one-half of the tellurium it contains. Gently heated in an atmosphere of chlorine it is changed into *bi*-chloride.

It is better prepared from the native telluret of silver, which can be subjected to a high temperature without risk of driving over any tellurium in the metallic state.

This compound, even when redistilled from metallic tellurium, is with difficulty obtained free from bi-chloride. The latter requires a somewhat higher temperature to volatilize it, and thus, by a cautious regulation of the heat, it may be obtained in the purest state.

Rose found it to be composed of

		Theory.	Experiment.
Chlorine	1 atom, = 4.426	= 35.45	= 37.77
Tellurium	1 atom, = 8.064	= 64.55	= 62.23
		12.49	100
			100 *

The difference between the theoretical and experimental results shows that a portion of bi-chloride was present.

It is curious to observe by what analogies the chlorides of sulphur, selenium, and tellurium are connected, and by what differences they are distinguished. Of sulphur we have one chloride, which is analogous in every respect to the di-chloride of selenium. But here the analogy stops, and there is no direct link to connect it with the chlorides of tellurium. But the higher, the solid chloride of selenium, is connected by an analogy equally close and striking with the bi-chloride of tellurium. These three bodies, therefore, form a natural group connected together like phosphorus and arsenic by many similar properties, and yet differing in others so widely as to afford the analytical chemist ample means of detecting the presence of each.

ART. XIV.—*The Address of his Royal Highness the DUKE OF SUSSEX, K. G. P. R. S. delivered to the Royal Society at the Anniversary Meeting on the 30th November 1831.*

GENTLEMEN,

THE period, provided by our statutes, has again come round, when your Officers and Council must be reconstituted by your authority; and I feel myself called upon, in conformity with the custom which has been sanctioned by my predecessors, to

* *Pogg. Ann.* xxi. p. 442.

address you upon such subjects connected with the Royal Society and its administration, as the events of the last year may have rendered proper to be noticed by me. But before I touch upon other topics, I feel anxious to say a few words upon my own position in the Society, and my views respecting it.

The chair of the Royal Society has been filled by a rare succession of illustrious men, and I feel proud that I have been judged worthy, upon any grounds, to occupy a situation which has become dignified by its association with the names of those who have conferred so much honour upon our country. It is indeed true that I can enter into no competition with such predecessors, as respects scientific knowledge, which my early education, my public occupations, and even the duties of my rank, have prevented me from cultivating and attending to that extent I could have wished : but I should do no honour to your kindness, which has placed me in this high and dignified station, if I should profess that I considered myself wholly inadequate to the efficient discharge of many at least of its public duties, or that I felt my occupation of this Chair was likely to prove injurious either to the credit of the Society, or to the advancement of science. If such, indeed, Gentlemen, were my own persuasion, I would not continue to fill this honourable post for another hour.

The ostensible duties, in fact, of your President, are chiefly ministerial : he is your organ to ask and to receive your decisions upon the various questions which are submitted to you ; and he is your public voice to announce them. Though he presides at the meetings of your Council, he possesses but one voice among many ; incurring an equal responsibility in common with every one of its members. He is your official representative in the administration of the affairs of the British Museum : he presides in your name, by virtue of your election of him, at the Board of Visitors at the Royal Observatory, as appointed by His Majesty's Warrant : he is your medium of communication with public bodies, and with the members of the Government upon the various subjects important to the interests of science, which are either submitted to your consideration, or which are recommended by you through your coun-

cil, for the consideration of others. For many of those functions I feel myself to be somewhat prepared by my habits of life, as well as by my public occupations : and for some of them more especially, if I may be permitted to say so, by that very rank in which Providence has placed me as a member of the Royal Family of this country ; for though it would be most repugnant to my principles and my wishes that the weight of my station should in any way influence the success of an application which it was either improper to ask or inexpedient to grant, I should feel it to be equally due to the dignity of this Society and to my own, that the expression of your opinions and of your wishes should experience both the respect and the prompt attention to which it is so justly entitled.

But while I should consider it my duty to exert the just authority of an English Prince in the assertion of your rights, and in the promotion of the success of those objects which you may intrust to my advocacy without these walls, yet within them I trust that I never have made, and that I never shall make use of it, either for the promotion of party purposes, or for the suppression of the candid, free and unbiassed expression of your opinions. In this Chair I appear as the Official Head of a Society comprising a great majority of the most distinguished men in science and in literature within the three kingdoms, and in this character alone I wish to be recognized ; and it is my most anxious desire to witness around me the free expression and interchange of opinions, subject to no restraints but such as are requisite for the regularity and well government of every numerous and mixed society.

I do not think it necessary, Gentlemen, to apologize to you for thus enlarging upon topics, which, though personal in some respect to myself, cannot be altogether destitute of interest to you ; inasmuch as it undoubtedly concerns you to understand distinctly the principles by which I have regulated my conduct hitherto whilst filling this Chair, and to which I shall continue to adhere in case I should be honoured by being re-elected to it. And I am the more anxious that they should be generally known, in consequence of some circumstances which attended my election last year. If any angry or uneasy feelings were called forth upon that occasion, I can assure you that I do

not, nor ever did, partake in them ; and it would be a source of the most heartfelt pride to me if I could witness their entire extinction in a cordial co-operation amongst all our members to promote the advancement of science and the common honour of our country ; to fulfil, in short, the solemn obligation imposed upon us individually and collectively by our charter, to promote the good of the Royal Society, established for the advancement of natural knowledge, and to pursue the ends for which it was originally founded.

Having ventured to say thus much upon a subject of some delicacy, though in no respect painful to myself, I trust that I may be permitted to add a few words more upon another topic which is nearly connected with it, and which is, to express my respect for the accomplished philosopher to whom I had the honour, I will not say misfortune, to find myself opposed last year. His name has been familiar to me from my earliest years, for it is that of one whom my royal father delighted to patronize, and which is inscribed in imperishable characters upon the great monuments of the universe, the knowledge of which he contributed so greatly to extend. I knew that venerable man when full of years and of honour, and I can well conceive the feelings of placid triumph and pride with which he must have contemplated the rising promise of his son. What the maturer fruits of that early promise have been, it is not necessary for me to state when addressing the members of this society : it is sufficient to say, that there is no one among the most illustrious men of England whom the concurrent voice of his countrymen would have pointed out as more worthy of the distinguished and peculiar mark of royal favour and approbation which he has so recently received than Sir John Herschel. Towards such a man I can entertain no feelings but those of admiration, respect, and good-will, and which I trust, if fed by a more intimate acquaintance, will ultimately lead to those of sincere friendship.

The labours of your Council during the past year have been more than commonly important, and have been directed to objects which deeply concern the welfare, good government, and general utility of our establishment. For the particulars of those labours I must refer you to the Report which has been

so ably drawn up by one of your Secretaries, Dr Roget, and which will be read to you by him at the conclusion of this Address. I trust, however, that in one particular I may be excused if I trespass upon the province of that Report; if with the natural partiality of an affectionate brother and a loyal subject, I venture to record the gracious expressions of His Majesty when he inscribed his royal name in our charter-book as the patron of the Royal Society, in the presence of the Council. His majesty then declared his gracious intention of continuing the same protection to this Society which had been extended to it by his royal predecessors; that his majesty had learnt from the professional pursuits of his early life to estimate the immense benefits which science had conferred upon this country in particular, and upon the world in general, by perfecting the art of navigation; that it had produced similar effects upon all the arts of life, however apparently remote from the source from which they flowed; that the progress of civilization amongst nations was generally coextensive with the improvements in science and the extent of its practical application; and that his majesty should feel it to be his duty, as the sovereign of these kingdoms, to aid by his encouragement the exertions of the Royal Society to fulfil the great objects of its foundation. His majesty concluded by recommending us in strong terms to cultivate friendly relations with the great scientific establishments of other countries, with a view to the free and liberal interchange of knowledge and discoveries. And here allow me, Gentlemen, to pause for a moment, with a view to remark that our Gracious Sovereign, in giving us this wholesome admonition relative to foreign scientific bodies, meant in a most delicate and dignified way, silently to convey to us his royal and paternal pleasure and advice as to the harmony and friendly intercourse which he wished us to maintain with all our national institutions, and more particularly amongst ourselves. Such sentiments, Gentlemen, are worthy of a King of England: and permit me further to observe, that it affords me additional pride and satisfaction that circumstances should have combined together so fortunately as to have made me the organ of such gracious communications between our royal patron and the Royal Society.

The Council, upon the same occasion, had the honour of presenting, in the name of the society, a dutiful and loyal address to her Majesty the Queen, who most condescendingly received them, and most graciously declared her intention of extending her support and protection to the Royal Society.

The list of Fellows whom the society has lost during the last year is more extensive than usual, and the time will not allow me more than to take a brief and passing notice of some of them, whose labours have brought them into a more immediate connection with this society and the great objects which it proposes to pursue.

Mr Abernethy was one of those pupils of John Hunter who appears the most completely to have caught the bold and philosophical spirit of investigation of his great master. He was the author of various works and memoirs upon physiological and anatomical or surgical subjects, including three papers, which have appeared in our Transactions. Few persons have contributed more abundantly to the establishment of the true principles of surgical or medical practice in those cases which require that minute criticism of the symptoms of disease, upon the proper knowledge and study of which the perfection of medical art must mainly depend. As a lecturer he was not less distinguished than as an author; and he appears to have possessed the art of fixing strongly the attention of his hearers, not less by the just authority of his opinions, than by his ready command of apt and forcible illustrations. He enjoyed during many years of his life a more than ordinary share of public favour in the practice of his profession; and though not a little remarkable for the eccentricities of his manners and an affected roughness in his intercourse with his ordinary patients, he was generally kind and courteous in those cases which required the full exercise of his skill and knowledge, and also liberal in the extreme when the affliction of poverty and privation was superadded to those of disease.

Captain Henry Foster was a member of the profession which, under all circumstances, is so justly celebrated for activity and enterprize, and which, when wanting the stimulus of war, has on many occasions lately distinguished itself by the zealous and successful cultivation of those studies and the practice of those

observations which are so essentially connected with the improvement of navigation. He accompanied Captain Basil Hall, in the *Conway*, in his well-known voyage to South America, and assisted him materially in his pendulum and other observations. He afterwards joined Captain Parry in the second of his celebrated voyages; and at Port Bowen and other stations within the Arctic Circle, he made, with the assistance of Captain Parry and others, a most valuable and extensive series of observations upon the diurnal variation, diurnal intensity and dip of the magnetic needle, and upon other subjects connected with terrestrial magnetism and astronomical refractions, which formed an entire fourth part of our *Transactions* for 1826, and was printed at the especial expence of the Board of Longitude. For these papers he received the Copley Medal; and the Lords of the Admiralty acknowledged their sense of the honour which was thus conferred upon the profession to which he belonged, by immediately raising him to the rank of Commander, and by appointing him to the command of the *Chanticleer* upon a voyage of discovery and observation in the South Seas. It was during the latter part of this voyage that he perished by an unfortunate accident; but I am happy to say that the public is not likely to lose altogether the benefit of his labours, and that he has left behind him an immense mass of observations of various kinds, which the Lords of the Admiralty have confided partly to this Society, and partly to the Astronomical Society, with a view to their publication in such a form as may best serve the interests of science, and may most tend to establish the character and fame of their lamented author.

The Reverend Fearon Fallows was a distinguished contemporary of Sir John Herschel at Cambridge, and throughout his life an ardent cultivator of astronomical science. In the year 1821 he was appointed Astronomer Royal at the Cape of Good Hope, to which place he immediately proceeded, though provided only with a small transit and an altitude and azimuth instrument, a clock, and a few other absolutely necessary appendages of an observatory. In the course of the two following years he completed a catalogue of 273 southern stars, which was published in our *Transactions* for 1824. The de-

lays which subsequently took place in the building of the observatory, which was not completed before 1828, and the want of those capital instruments which were required to put it into complete operation, although they did not interrupt or check either the industry of his research or the accuracy of his observations, yet by making them necessarily imperfect, deprived them of a very considerable part of their value.

When the mural circle at last arrived, and when he at length imagined himself in possession of the means of effecting the great object of his ambition, by making the catalogues of the stars of the southern hemisphere rival, in accuracy and completeness, those of the northern, he found new difficulties meeting him in the derangements occasioned in so large an instrument, by embarking, disembarking, and fixing it, thus producing errors which were nearly irremediable in the absence of the original maker, or of any superior artist. In the midst of these harassing discouragements he was attacked by severe illness, and at the same time deprived of his assistant by a similar cause, yet even under these afflictions he continued true to his duty; and in a letter to one of his friends a short time before his death, he describes himself as being carried daily in a blanket by his servants from his sick room to the observatory for the purpose of winding up his clocks and chronometers. His disease at last assumed the form of an incurable dropsy, and he died a short time before his intended embarkation for England, whither at last he had reluctantly consented to return, when his recovery at the Cape was pronounced to be hopeless.

In the course of the year 1829 he made, in conjunction with Captain Ronald, and Mr Johnstone, a very complete series of pendulum observations, which were published in our Transactions for the year 1830: and the Lords of the Admiralty are in possession of a very extensive series of astronomical observations made during the last seven years of his life, which it is to be hoped that, before long, they will cause to be given to the public.

Lieutenant-Colonel Macdonald, son of the celebrated Flora Macdonald, besides many professional and other works, was also the author of two papers in our Transactions for the years 1796 and 1798, containing observations upon the diurnal vari-

ation and dip of the magnetic needle made at Fort Marlborough in Sumatra, accompanied likewise by some observations upon their causes.

Mr Thomas Greatorex, the well-known musician, was the author of a paper on the measurement of the heights of mountains. He was a person of great modesty and simplicity of character, and possessed a knowledge of some branches of mathematics and of natural philosophy which is rarely met with in the members of his profession.

Sir Thomas Frankland, as long ago as the year 1795, was the author of a short paper in our Transactions on the welding of cast steel and iron.

Mr William Strutt of Derby was the author of those great improvements in the construction of stoves, and in the economical generation and distribution of heat, which have of late years been so extensively and so usefully introduced in warming and ventilation of hospitals and public buildings. He possessed a very great knowledge of practical mechanics, and employed himself through the whole course of a very active life in the furtherance of objects of public utility.

Dr Parkinson, Archdeacon of Leicester, gained the highest honours at Cambridge, and was the author of a treatise on mechanics. In his early life he was employed in conjunction with Israel Lyons and others, in the formation of the tables requisite to be used with the *Nautical Almanac*.

Dr Sims was a very zealous cultivator of botanical science, and continued for many years the publication of Curtis's *Botanical Magazine*.

Dr Ferris, besides other professional publications, was the author of a work entitled "*A General view of the establishment of Physic as a Science in England.*"

The Rev. William Holwell Carr was a gentleman of refined and cultivated taste, and a liberal patron of the fine arts; he has established no slight claim upon the gratitude of his country by the bequest of his collection of exquisite pictures to the British Museum, whose Council have thought it most advisable, for their better preservation and security, as well as for the furtherance of that gentleman's views in making such a

magnificent present to the nation, to deposit them in the British Gallery.

The Earl of Darnley was a liberal patron of the Fine Arts, and a zealous friend of all useful public institutions: and he gave a most convincing proof of the interest which he felt in the promotion of natural knowledge, by the formation and maintenance of a noble collection of rare and curious plants and animals.

Mr Thomas Hope, the justly celebrated author of *Anastasis*, and Dr Magee, Archbishop of Dublin, author of the great work upon the atonement, are names not likely to be soon forgotten in the literary history of this country; but they require no further notice from me, as their labours are altogether foreign to the pursuits of this society.

The only foreign member whose death we have to record is the celebrated Sömmerring, who died lately at Frankfort, his native city, full of years and honour. His numerous and most splendid anatomical works, particularly those on the different organs of sense, have long placed him at the head of the anatomists of Germany, and probably of Europe.

I cannot conclude this Address, Gentlemen, without again requesting you to accept my assurances of the sense which I entertain of the high honour of presiding over this Society, and of my determination to promote its interests to the utmost of my power and ability, in case it should be your pleasure to confide them again to my keeping, by electing me a second time to fill this Chair.

ART. XV.—*Analysis of Gmelinite or Hydrolite*. By THOMAS THOMSON, M. D. F. R. S., &c.

THIS mineral seems to have been first discovered by Leman, in the cavities of amygdaloid rocks, in the Vincentine. These specimens were analyzed by Vauquelin, under the name of Sarcolite; and Haüy considered them as mere varieties of analcime. Some years ago the mineral was discovered in the county of Antrim, Ireland, lodged in amygdaloidal rocks, precisely as in the Vincentine. The specimens in my possession were pro-

cured from Patrick Doran, an Irish mineral-dealer, who had collected them in this locality. Dr Brewster gave an account of the physical properties of this mineral in his *Scientific Journal*, under the name of *Gmelinite*; and Haidinger has described it under the same name, in an appendix added to his English translation of Moh's *Mineralogy*.

Colour snow-white.

All the specimens which I have seen are in double six-sided truncated pyramids, with a short six-sided prism between them. The inclination of the faces of the one pyramid upon those of the other, according to Dr Brewster's measurement, is $83^{\circ} 36'$.

Translucent.

Hardness 3.5. Scratches calcareous spar, but not fluor-spar.

Lustre vitreous.

Specific gravity 2.054.

Very easily frangible.

Before the blowpipe, swells out and assumes the appearance of an enamel; but does not fuse into a transparent glass.

When exposed to a red heat, it gives out water, and nothing else, and loses 29.866 per cent. of its weight.

I subjected it to analysis; but, as the quantity of it in my possession only amounted to 5.3 grains after ignition, it will be necessary to state the steps of the analysis, to enable the reader to judge of the degree of confidence to which my experiments are entitled.

The 5.3 grains of the ignited mineral, after having been reduced to a fine powder, were intimately mixed with 30 grains of carbonate of barytes, in a platinum crucible, and the mixture was exposed to a strong red heat, and kept at that temperature for an hour. The whole was then dissolved in dilute muriatic acid. The undissolved portion having the appearance of hydro-lite undecomposed, was mixed with 30 grains of carbonate of barytes, and kept in a strong heat for two hours. It was then dissolved in dilute muriatic acid. A few flocks remained undissolved; but they were light and loose, indicating that they had been acted upon by the barytes. The two solutions were mixed together, and evaporated to dryness in a porcelain basin. The dry mass was digested for some time in water acidulated with muriatic acid. The whole was then thrown on the filter, to

separate the undissolved silica from the solution. The silica on the filter being washed, dried, and ignited, weighed 4 grains. It was laid aside for examination.

The muriatic acid solution was neutralized by caustic ammonia added slightly in excess. A brown precipitate fell, weighing, after ignition, 1.08 grains. Being digested in muriatic acid, it left undissolved 0.055 grains of a grey matter, which, tested by the blowpipe, proved to be silica very slightly tinged with iron.

The muriatic solution thus freed from silica was mixed with potash-ley in considerable excess, and heated in a flask. There was precipitated a quantity of peroxide of iron, weighing, after edulcoration, drying, and ignition, 0.44 grain. The potash-ley had dissolved the alumina of the precipitate, which obviously amounted to 0.585 grain. Thus the brown precipitate thrown down by caustic ammonia was composed of,

Silica,	-	-	0.055
Peroxide of iron,	-	-	0.440
Alumina,	-	-	0.585
			1.080

The muriatic solution was now mixed with a sufficient quantity of carbonate of ammonia, to throw down the whole of the barytes. The filtered liquid was evaporated to dryness, and exposed to a graduated heat, to drive off the ammoniacal salts. The residue was found to contain lime derived from the filter. To get rid of it, I added some carbonate of ammonia, heated the liquid in a flask, then left it in a small glass cylinder till the carbonate of lime subsided; drew off the clear supernatant liquid by a sucker, edulcorated the carbonate of lime by distilled water, which was drawn off in like manner by a sucker. The liquid was evaporated to dryness in a platinum vessel, and the ammoniacal salt driven off. There remained behind a little saline matter, which weighed, after ignition, 0.84 grain. It was soluble in water, and the aqueous solution was abundantly precipitated by muriate of platinum. Hence the salt was chloride of potassium, and contained 0.41 potassium, equivalent to 0.53 potash.

The 4 grains of silica, obtained at the beginning of the analysis, were mixed with thrice their weight of anhydrous carbonate of soda, and exposed to a strong heat in a platinum crucible. The mass which had undergone fusion, was dissolved in muriatic acid, and the solution was evaporated to dryness. The dry residue was digested in dilute muriatic acid, and thrown upon a filter, to separate the silica. The silica, after edulcoration, drying, and ignition, weighed 2.96 grains. It was a fine white powder, and was perfectly pure.

The muriatic solution, thus freed from silica, was mixed with caustic ammonia slightly in excess; a greyish-brown precipitate fell, weighing, after ignition, 0.58 grain. By solution in muriatic acid, and mixing the solution with caustic potash in considerable excess, it was resolved into

Peroxide of iron,	0.185
Alumina, -	0.395
	.580

Thus, from the 4 grains of the siliceous-looking matter, separated from the hydrolite, when it was treated with carbonate of barytes and muriatic acid, were obtained,

Pure silica, -	2.96
Peroxide of iron,	0.155
Alumina, -	0.395
	3.54
Loss, -	0.46
	4.00

This loss could have been owing to nothing but the presence of a little potash in the siliceous matter (the liquid was carefully examined, but nothing found), which I could not obtain, because I had fused the 4 grains of siliceous matter with carbonate of soda.

If we now add together all the constituents, we shall find that 5.3 grains of anhydrous hydrolite are composed of

Silica,	-	3.015
Alumina,		0.980
Peroxide of iron,		0.625
Potash,	-	0.530
		4.95

As hydrolite contains 29.866 per cent. of water, it is obvious, that if the 5.3 grains analyzed had retained their water, the weight would have been 7.53 grains. Consequently the constituents, according to the preceding analysis, considering the loss as potash, and converting the peroxide of iron into protoxide, are as follows :

Silica,	-	8.015	or	39.896
Alumina,		0.980		12.968
Protoxide of iron,		0.5625		7.443
Potash,	-	0.7425		9.827
Water,	-	2.2050		29.866
		7.5050		100

This is equivalent to

14 atoms silica,
 4 atoms alumina,
 1 atom protoxide of iron,
 1 atom potash,
 18 atoms water.

We may therefore consider hydrolite as a compound of

4 atoms bisilicate of alumina,
 1 atom bisilicate of potash,
 1 atom quatersilicate of iron,
 18 atoms water.

So that every integrant particle of the mineral is combined with three atoms of water.—*Edin. Trans.* vol. xi. p. 448.

ART. XVI.—*On an Inequality of long Period in the Motions of the Earth and Venus.* By GEORGE BIDDELL AIRY, A. M. Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.

THE author had pointed out, in a paper published in the *Philosophical Transactions* for 1828, on the corrections of the elements of Delambre's Solar Tables, that the comparison of the corrections of the epochs of the sun and the sun's perigee, given by the late observations, with the corrections given by the observations of the last century, appears to indicate the existence of some inequality not included in the arguments of those tables. As it was necessary, therefore, to seek for some inequality of long period, he commenced an examination of the mean motions of the planets, with the view of discovering one whose ratio to the mean motion of the earth could be expressed very nearly by a proportion of which the terms are small. The appearances of Venus are found to recur in very nearly the same order every eight years; some multiple, therefore, of the periodic time of Venus is nearly equal to eight years. It is easily seen that this multiple must be thirteen; and consequently eight times the mean motion of Venus is nearly equal to thirteen times the mean motion of the earth. The difference is about one 240th of the mean annual motion of the earth; and it implies the existence of an inequality of which the period is about 240 years. No term has yet been calculated whose period is so long with respect to the periodic time of the planets disturbed. The value of the principal term, calculated from the theory, was given by the author in a postscript to the paper above referred to. In the present memoir he gives an account of the method of calculation, and includes also other terms which are necessarily connected with the principal inequality. The first part treats of the perturbation of the earth's longitude and radius vector; the second of the perturbation of the earth in latitude; and the third of the perturbations of Venus depending upon the same arguments.

The computations of the quantities themselves being effected by means of algebraical equations of great complexity, and of

numerical calculations of considerable length, which afford in themselves no ready means of verifying their accuracy, the author has been under the necessity of examining closely every line of figures before he proceeded to another. Upon the whole, he is certain that there is no error of importance in the numbers he obtained ; and that the only probable source of error is the inevitable rejection of figures beyond a certain place of decimals.

In concluding this investigation, the most laborious, probably, that has yet been made in the planetary theory, he remarks that the term in question is a striking instance of the importance to which terms, apparently the most insignificant, may sometimes rise. As an illustration of the magnitude of the errors which might under other circumstances have arisen from the neglect of this term, he further observes, that if the perihelion of Venus and the earth had opposite longitudes, and if the line of nodes coincided with the major axis, the eccentricities and inclination having the same values as at present, the coefficient of the inequality in the epoch would be $8''.9$, and all the other terms would be important. A very small increase of the eccentricities and inclination would double or treble these inequalities.

ART. XVII.—*On some new Compounds of the Chlorides of Platinum.* By Professor ZEISE of Copenhagen.

Hydro-carburetted Chloride of Platinum.

THE chloride of platinum does not dissolve in alcohol. When heated, however, it is decomposed by it and converted into a black powder,—the supernatant alcohol becoming acid, but remaining colourless. The bi-chloride dissolves readily in alcohol ; but the solution becomes acid, deposits a part of the platinum in the form of a black powder, and retains the other part in solution in a new state of combination, which has been ably investigated by Professor Zeise of Copenhagen.*

If the bi-chloride be dissolved in ten times its weight of alcohol, sp. grav. = .832, and the filtered liquid evaporated to one-sixth, the addition of sal-ammonia causes no precipitate, showing that the solution contains no common bi-chloride of

* Poggendorf's *Annal.* xxi. p. 506.

platinum. Evaporated to dryness it leaves a brown mass, which, treated with water, gives a yellow solution and leaves a brown substance undissolved. The yellow solution evaporated to dryness over caustic potash in vacuo, gives a yellow mass, which is the hydro-carburetted chloride of platinum, and may be farther purified by again dissolving and evaporating.

It may also be prepared by adding to a concentrated solution of hydro-carburetted chloride of platinum and ammonia; a solution of neutral chloride of platinum, drop by drop, as long as a precipitate falls, filtering and evaporating. By this process it is obtained in the purest state.

When pure this salt is citron-yellow; becoming brown and black in the light. It does not deliquesce, and is slightly soluble in water and alcohol; giving yellow solutions. Heated it gives off muriatic acid and carburetted hydrogen, and leaves a black mass which burns when heated in the air, and leaves metallic platinum.

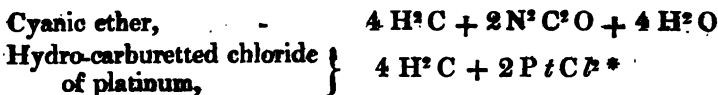
The aqueous solution when boiled gives off much inflammable gas, and deposits almost the whole of its platinum in the metallic state. Nitrate of silver added to the solution throws down chloride of silver; a black powder then falls, and after this is all separated by boiling, a farther addition of nitrate of silver causes a new deposit of chloride of silver.

From this phenomenon, Zeise inferred that one portion of the chlorine in this salt existed in the state in which it is found in the chlorides, and the other in the same state as in chloric and hydro-chloric ether, and, therefore, that the salt was probably a compound of chloride of platinum, and hydro-chloride of carbon (chloric ether). But in a later paper * he prefers considering it as a compound of neutral chloride of platinum, with olefiant gas, similar to the different species of ether in the following formulæ.

Oxalic ether,	-	$4 \text{ H}^2 \text{ C} + \text{C}^2 \text{ O}^5 + \text{H}^2 \text{ O}$
Sulpho-vinic acid,	-	$4 \text{ H}^2 \text{ C} + \ddot{\text{S}} + \text{H}^2 \text{ O} \dagger$
Muriatic (hydro-chloric) ether,		$4 \text{ H}^2 \text{ C} + 2 \text{ H C l}$
Chlorine ether,	-	$3 \text{ H}^2 \text{ C} + \text{C C l}^2$

* Pogg. xxi. p. 543.

† Messrs Wohler and Liebig consider the sulpho-vinic acid as most probably a compound of hydrated sulphuric acid and ether.—*Ann. de Chim.* xlvii. p. 425.



Zeise did not determine the composition of this salt by direct analysis, but he inferred it from that of the following compound salts to consist of

1 atom platinum	=	1233.2600	=	66.528
2 — chlorine	=	442.6500	=	23.879
2 — carbon	=	152.8750	=	8.247
4 — hydrogen	=	24.9592	=	1.346
		1853.7442		100

The different species of ether are considered by Dumas and Boullay as salts of carburetted hydrogen, (olefiant gas,) and in this platinum salt Zeise considers the chloride of platinum to act the part of an acid, and the olefiant gas that of a base. This view coincides also with that of Bonsdorf, who considers the double metallic chlorides as salts in which the chlorides of the electro-positive acts the part of an acid to that of the electro-negative metal.

Hydro-Carburetted Chloride of Platinum and Potassium.

If the alcoholic solution of the bi-chloride of platinum be concentrated till a strong solution of chloride of potassium produce no precipitate, diluted with four times its weight of water, filtered and digested upon a quantity of dry powdered chloride of potassium, equal in weight to one-fourth of the dry chloride of platinum employed, till the whole is dissolved and the solution gently evaporated, there are deposited on cooling a crop of beautiful yellow or brownish-yellow crystals, which may be freed from excess of acid by again dissolving and crystallizing. This is Zeise's hydro-carburetted chloride of platinum and potassium.† The salt forms beautiful rhombic prisms, having an angle of $103^{\circ}.58'$, while the angle formed by the lateral and terminal planes is $112^{\circ}.5'$. They belong to the hemiprismatic system of Mohs. They are citron-yellow, transparent, have an

* These formulæ are all expressed according to Berzelius's atomic weights.

† This salt was first formed and described by Berzelius, but not analyzed. — *Arsberättelse*, 1829, p. 159.

astringent metallic taste, redden litmus, dissolve in five times their weight of water, are nearly as soluble in alcohol, and give a yellow solution. Exposed to light and air they become externally of a black colour. By a gentle heat they lose water. —At a high temperature they give off muriatic acid and an inflammable gas, but are not susceptible of entire decomposition by heat. Zeise obtained the platinum by heating the anhydrous salt in a porcelain crucible with carbonate of soda,—estimated the chlorine by dissolving the common salt formed, and precipitating by nitrate of silver, and the carbon and hydrogen by a combustion with oxide of copper. By this process he obtained the following composition :

2 atoms Platinum,	2466.5200	53.157 per cent.
4 ——— Chlorine,	885.3000	19.079
1 ——— Potassium,	489.9160	9.539
2 ——— Chlorine,	442.6500	10.558
4 ——— Carbon,	305.7440	6.589
8 ——— Hydrogen,	49.9184	1.076
2 ——— Water,	224.9600	
	4865.0084	100

And the formula for the dry salt is

$4 \text{ H}^2 \text{ C} + 2 \text{ Pt C l}^2 + \text{K C l}^2$, or, as Zeise prefers to consider it, $(4 \text{ H}^2 \text{ C} + \text{Pt C l}^2) + (\text{K C l}^2 + \text{Pt C l}^2)$

Hydro-Carburetted Chloride of Platinum and Ammonium.

If common salt be employed instead of the chloride of potassium in the foregoing process, a solution is obtained which does not crystallize, but if sal-ammoniac be substituted, beautiful prismatic crystals are obtained, similar to those of the potassium salt, and leaving when strongly heated only metallic platinum. The composition of the dry salt is $(4 \text{ H}^2 \text{ C} + \text{Pt C l}^2) + (\text{N}^2 * \text{ H}^5 \text{ C l}^2 + \text{Pt C l}^2)$, and it contains $1\frac{1}{4}$ atoms water.

Ammonia Hydro-Carburetted Chloride of Platinum.

When caustic or carbonate of ammonia is added to the solution of any of the above hydro-carburetted chlorides, or when

* This is Berzelius's expression for Ammonium, that for Ammonia is $\text{N}^2 \text{ H}^6$.

caustic potash is added to the last-mentioned, a bulky yellow precipitate falls, which Zeise finds to be a distinct compound salt, in which caustic ammonia takes the place of the sal-ammoniac in the salt last described. The formula is $(4 \text{ H}^2 \text{ C} + \text{P t C } \bar{\text{P}}) + (\text{N}^2 \text{ H}^6 + \text{P t C } \bar{\text{P}})$

The last member of the formula represents the composition of a green powdery compound formerly described by Magnus, and is similar to the compounds of ammonia with the fluorides of silicon and boron.

ART. XVIII.—*Summary of the State of the Barometer, &c. in Kendal, 1831.* By Mr SAMUEL MARSHALL. Communicated by the Author.

1831.		Barometer.		
		Max.	Min.	Mean.
January,	-	30.40	29.08	29.71
February,	-	30.20	28.87	29.59
March,	-	30.40	28.88	29.66
April,	-	30.46	28.57	29.54
May,	-	30.10	28.75	29.66
June,	-	29.98	29.16	29.69
July,	-	30.16	29.30	29.76
August,	-	30.10	29.38	29.73
September,		30.00	29.33	29.66
October,	-	30.14	29.05	29.49
November,		30.26	28.93	29.55
December,		30.27	28.55	29.47
Means, &c.		30.20	28.98	29.62

1831.	Thermometer.			Quantity of rain.	No. of rainy days.	Prev. wind.
	Max.	Min.	Mean.			
January,	48°	18°	33°.37	1.619	6	N.W.
February,	50.5	19	38.14	8.208	17	S.W.
March,	55	31	43.20	6.028	17	W.
April,	61	26.5	47.82	2.433	11	W.
May,	- 71	28.5	51.11	.721	8	N.E.
June,	- 70	40	58.06	2.682	16	W.
July,	- 73	44	60.50	4.081	17	W.
August,	73	44	60.84	3.899	14	W.

September, 66°	38°	54°.92	6.393	15	W.
October, 66	37.5	53 .85	11.812	26	W.
November, 55	15	39 .76	8.560	18	W.
December, 52	29	41 .68	4.980	21	W.
Means, &c. 61.70°	30.87°	48°.60	61.416	186	W.

ONE of the most prominent features of the weather in this year is the dryness of the summer months. This may not appear very evident from inspecting the column of rainy days in the preceding table. It may be explained, however, by stating, that in many of these days a very trifling quantity of rain was measured. The plan pursued in these observations is to note down every day as a rainy one on which any quantity of rain has fallen equal to a hundredth part of an inch, but if less, it is added to that of the following day. From the middle of March to the middle of September, the weather was decidedly dry. October was by far the wettest month, as there were but five days on which some rain did not fall, and sometimes large quantities. On the 9th of February 2.441 inches of rain were measured, which fell in the preceding twenty-four hours; and on the 25th September, 2.160 in a similar period. On these days, the river Kent rose so high as to inundate the lower part of the town, the former to a greater height than can be remembered by any one in the town.

The aurora borealis was seen very frequently during the first five months in the year, and a few times in September and October, since which time it has not been noticed, probably owing to the cloudy state of the atmosphere. It was remarked that after a brilliant display of streamers, there was generally a change in the weather.

The mean of the barometer is about equal to that of other years; that of the thermometer greater. The number of rainy days exceeds the average of the preceding eight years. The seed and harvest months have been remarkably favourable for the labours of the agriculturalist, and the crops were mostly well got, particularly those of grain.

The quantity of rain is greater than in any year since 1824, in which year, and the two preceding years, it was about 62 $\frac{1}{4}$ inches.

ART. XIX.—*On the advantage of a Collection of Numbers, to be entitled the Constants of Nature and of Art.* By CHARLES BABBAGE, Esq. A. M. F. R. S. & L. & E. Lucasian Professor of Mathematics in the University of Cambridge. In a Letter to Dr BREWSTER.

MY DEAR SIR,

AMONGST those works of science which are too large and too laborious for individual efforts, and are therefore fit objects to be undertaken by united academies, I wish to point out one which seems eminently necessary at the present time, and which would be of the greatest advantage to all classes of the scientific world.

I would propose that its title should be "*The Constants of Nature and of Art.*" It ought to contain all those facts which can be expressed by numbers in the various sciences and arts. A better idea will be formed by giving an outline of its proposed contents, and it may perhaps be useful to indicate the sources whence much of the information may be drawn.

These constants should consist of

1. All the constant quantities belonging to our system ;—as distance of each planet,—period of revolution,—inclination of orbit, &c.—proportion of light received from sun,—force of gravity on surface of each.

These need not be further enumerated, as they have already been collected, and need only be copied.*

2. The atomic weights of bodies.

These may be taken from Berzelius, Thomson, or Turner.

The proportions of the elements of various compounds ;—Acids with bases,—metals with oxygen, &c.

These may be taken from the best treatises on chemistry.

3. A list of the metals, with columns containing specific gravity,—Elasticity,—tenacity,—specific heat,—conducting power of heat,—conducting power of electricity,—melting point,—refractive power,—proportion of rays reflected out of 1000—at an incidence of 90°.

3. List of specific gravities of all bodies.

* A work of this kind, embodying the results of science, has been projected for some time by M. Poggendorff of Berlin, and a specimen of it may be seen in his *Annalen*, xxi. p. 609.—J.

4. List of Refractive indices.

——— Dispersive indices.

——— Polarizing angles.

——— Angles formed by the axes of double refraction in crystals.

These may be extracted from the writings of Brewster, Mitscherlich, Herschel, Biot.

5. Number of known species of mammalia—birds—reptiles—fishes—mollusca—worms—crustacea—insects—zoophytes.

These classes might be further subdivided.

Additional columns should show how many of each are found in a fossil state, and the proportion between the fossils of existing and extinct species.

6. List of mammalia, containing columns expressing height—length—weight—weight of skeleton,—weight of each bone—its greatest length—its smallest circumference—its specific gravity—also the number of young at a birth—the number of pulsations per minute, whilst the animal is in repose—the number of inspirations in the same circumstances—period of blindness after birth—period of sucking—period of maturity—temperature,—average duration of life—proportion of males to females produced.

It would be desirable to select some bone for the unity of weight and perhaps of measure, and to give the proportion of all the other bones to this standard one. The numerical relations thus established might perhaps in some cases identify the sexes, or even the races of the human species, when only a few bones were found. It would also be highly interesting to compare the relative weight of the bones of persons employed in different trades, and of persons dying from certain constitutional diseases.

7. Of Man. Average weight at various periods of existence—height of do.—tables of mortality in various places—average duration of reigns of sovereigns,—proportions of the sexes born under various circumstances,—proportion of marriages under various circumstances,—quantity of air consumed per hour,—quantity of food necessary for daily support,—average proportion of sickness amongst working classes,—proportion of persons dying from different diseases.

Many of these facts may be found in the writings of Villermé, Quetelet, Bailly, Milne, &c.

8. Power of Man and Animals.

A man labouring ten hours per day will saw () square feet of deal—ditto () elm—ditto () oak, &c.—ditto Portland stone—ditto Purbeck—Days labour in mowing, ploughing—&c. &c. every kind of labour—Raising water one foot high—horse do.—ox or cow do.—camel.

Power of steam engines in Cornwall.

Inclination of a road, both in degrees and number of feet, &c. or of a base on which carriages and horses can trot—walk,—on which horses cannot ascend—on which man cannot,—on which a cart cannot ascend.

9. Vegetable kingdom. Number of species known of monocotyledonous plants—number of species of dicotyledonous plants.

Number of species of the various natural groups.

Additional columns should show the number of species known in a fossil state, together with that of extinct fossil species.

Also average weight of vegetable produce of one acre in a year, when under different modes of cultivation,—hay—straw—wheat,—turnips,—and mangel wurzel,—potatoes—clover, &c.—produce of timber per acre.

10. Tables of the geographical distribution of animals and of plants,—of the average period of maturity and decay in various woods,—increase in weight annually at different periods,—weight of potass produced from each—proportion of heat produced by burning given weight.

11. Atmospheric phenomena. Weight of air above a square inch,—square foot,—an acre,—a square mile of the earth's surface, barometer at 30 in. Weight of oxygen, of nitrogen, of carbonic acid, above the same spaces, under the same circumstances.

Weight of water in vapour above ditto at various degrees of hygrometer. Depth of rain falling annually at various places, in inches,—columns for number of year's observation,—mean temperature,—mean height of barometer,—height of places above the sea,—drainage of surface-water for one, two, three, to ten inches, from each square of 100 feet side, each acre, or square mile, expressed in cubic feet, in gallons, and in hogsheds, water discharged per 1" or 1', per hour or per day, under va-

rious circumstances, as found by experiment,—velocity of rivers and torrents to carry stones of given weight.

12. Materials.—Height to which a column of any substance used in building may be carried before the lowest layer is crushed,—weight necessary to crush a cubic inch of each,—weight of cubic foot or cubic yard. Angles at which sand, gravel of various sized pebbles, snow, &c. support themselves. Strength necessary to pull asunder various woods,—bars of metal of various dimensions,—weight to break ropes and chains of various sizes,—column for weight to be safely borne by them,—friction under various circumstances,—resistance of fluids.

Weight of coal to burn 10 bushels of lime,—weight of ashes to burn 10,000 brick,—of coak to make ton of wrought iron,—tallow to make soap, &c.—and constants in all trades.

See Rennie, Tredgold, Prony, Eytelwein, Venturi, &c.

13. Velocities.—Arrow, musket-ball at several distances, cannon-ball, sound, telegraph, light,—birds.

Days journey. Man, horse, heavy waggon, stage-coach, mail-coach, camel, elephant, steam-carriage, steam-boat, balloon, greatest—average passage Liverpool to New York, &c.—of steam-boats, Dublin to Liverpool,—London to Edinburgh, &c.

14. Length of all rivers,—water discharged per hour.—Seas—proportion of water to land on globe,—area of all seas and lakes in square miles,—areas of all islands and peninsulas and continents,—heights of mountains,—depth of mines from surface,—quantity of water pumped out of mines.

Heights of above 7000 points in Europe may be found in *Orographie*, the third volume of the *Transactions of the Geographical Society of Paris*.

15. Population, extent in square miles, revenue, &c. of kingdoms,—births, deaths, marriages,—rate of increase,—population of great towns.

16. Buildings.—Height of all temples, pyramids, churches, towers, columns, &c. ;—also all single stones, as obelisks, and area covered by ditto,—area of all great public buildings. Dimensions of all columns in ancient temples,—lengths of all bridges,—of span of each arch, and height, also breadth of piers.

Such tables may be found in Wiebeking, *Architecture Civile*, in ———

17. Weights, measures, &c.—Factors and their logarithms to convert all money of every country into English pounds sterling.

Factors and their logarithms to convert weights of every country into English pounds avoirdupois.

————— foot and ell measures
in every country into English feet.

————— measures of area,
acres, &c. into English acres.

————— liquid measures in
every country into English imperial gallons.

These are already collected in several works of Löhmann of Dresden. See also *Universal Cambist*.

18. Tables of the frequency of occurrence of the various letters of the alphabet in different languages,—of the frequency of occurrence of the same letters at the beginnings or endings of words,—as the second or as the penultimate letters of words,—of the number of double letters occurring in different languages,—of the proportion of letters commencing surnames amongst different nations.

See Quetelet, *Journal*, also *Dissertatio inauguralis Mathematica de literarum proportionibus*, F. J. Adelman, Bruxelles, 1829.

19. Table of number of books in great public libraries at givendates,—number of students at various universities. Observatories of the world,—Transit, length of diameter of object-glass, maker,—Circle, length of telescope, aperture, diameter of divided circle, maker.

It would be desirable to give the date of the different eras by which time is computed, and perhaps tables of the reigns of sovereigns. Also a chronological table,—at least of scientific discoveries and their authors.

In the above enumeration, which is far from complete, some few of the uses of such a volume are noticed, others will present themselves to every reader, and probably many unexpected ones will arise. The facts being all expressed in numbers, if printed in a small type and well arranged, would not occupy

a large space. Most of the constants mentioned in this list already exist, and the difficulty of collecting them would consist chiefly in a judicious selection of those which deserved the greatest confidence. The labour of extracting them from a great variety of volumes, and of reducing the weights and measures of other countries to our own, could be performed by clerks. To any individual who might attempt it, it must be a work of great labour and difficulty, and there are few persons possessing the varied knowledge which such a task implies whose talents might not be differently employed with more advantage to science. It is also certain that such an assemblage of facts, emanating from the collected judgment of many, would naturally command greater attention than if it were the produce of any single individual, however eminent.

It appears, then, that such a work is particularly fitted to be the production of a body of men of science, and I would appeal to the great academies of Europe whether they would not, by combining in one volume so vast a collection of facts, confer an important advantage upon science and upon all who are occupied with its pursuits. I would suggest that three of the academies of Europe, perhaps the Royal Society, the Institute of France, and the Academy of Berlin, should each publish at intervals of six years their own table of the **CONSTANTS OF NATURE AND ART**. Thus these publications might succeed each other at intervals of two years, and the man of science would always be able to refer to the most recent determinations of the constants he employs.

In order to execute the work, sub-committees of one or two persons must be appointed to each department, who should be directed in the first instance to prepare the outline of the constants they propose to insert. These views should then be considered and classed by a small committee, consisting of persons of general views and of various knowledge. The sub-committee should then collect and reduce to certain standards the constants committed to them, and the whole should be printed under the general superintendence of the committee, but each part should be specially revised by its own sub-committee.

A preface should be prepared, stating as shortly as possible

the reasons for preferring or rejecting particular experiments or observations, and also, generally, the degree of accuracy the several subjects admit of. A good and concise system of references should be made to all the authorities for the numbers given. Whoever should undertake the first work of this kind would necessarily produce it imperfect; partly from omission, and partly from the many facts connected with natural history, which, although measured by number, have not yet been counted.

But this very deficiency furnishes an important argument in favour of the attempt. It would be desirable to insert the heads of many columns, although not a single number could be placed within them,—for they would thus point out many an unreaped field within our reach, which requires but the arm of the labourer to gather its produce into the granary of science.

It is, however, to be hoped that no fear of the imperfection of a first attempt will deter either any individual or any body of men from an immediate endeavour to produce a work fraught with so many advantages to knowledge. The task of revising it at each period of six years will be comparatively easy, and the discussions of new observations or additional experiments made during those intervals, will have an admirable effect in exciting the ambition of the inquirers, to bestow such care as shall claim for their results a place in the volume, in which the academy shall record the condensed expression of the knowledge of their age and nation.

If I should be successful in inducing any scientific academy to enter in the task, I am confident that many a weary hour, now wasted in the search for existing knowledge, will be devoted to the creation of new, and that it will thus call into action a permanent cause of advancement towards truth, continually leading to the more accurate determination of established facts, and to the discovery and measurement of new ones.—I remain, my dear Sir, very sincerely yours,

C. BABBAGE.

DORSET STREET, MANCHESTER SQUARE,

22d Feb. 1832.

To Dr BREWSTER.

ART. XX.—*On the Temperature and Saltness of the Waters of the Ocean at different Depths.* By L. LENZ.

M. LENZ, naturalist to the expedition of Kotzebue, made a series of well-conducted experiments on the temperature and saltness of the ocean in different latitudes and at various depths. The instrument he employed for ascertaining the temperatures was an improvement upon that of Hales, being a large cylinder closed at both ends by valves opening inwards, to one of which was attached a thermometer, and surrounded by a highly non-conducting substance.—The results are contained in the following table :—

Time of observation.	Lenz.	Places.		Depth in toises.*	Temperature.	
		Lat. N.	Long. W.		at sur- face.	at depths indic.
1 1823. Oct. 10,	Atlant. Oc.	7° 21'	21° 59'	539	25°, 80 C.	2°, 20
2 1824. May 18,	South Sea.	21 14	196 1	140,7	26,40	16,36
3 " "	"	"	"	413,0	"	3,18
4 " "	"	"	"	665,1	"	2,92
5 " "	"	"	"	914,9	"	2,44
6 1825. Feb. 8,	"	25 6	155 58	167	21,50	14,00
7 Aug. 31,	"	32 6	136 48	89,8	21,45	13,35
8 " "	"	"	"	214,0	"	6,51
9 " "	"	"	"	459,2	"	3,75
10 " "	"	"	"	592,6	"	2,21
11 1826. Mar. 6,	Atlant. Oc.	32 20	42 30	1014,8	20,86	2,24
12 1825. Aug. 24,	South Sea.	41 12	141 58	205,0	19,20	5,16
13 " "	"	"	"	512,1	"	2,14
14 1826. Mar. 24,	Atlant. Oc.	45 53	15 17	197,7	14,64	10,36
15 " "	"	"	"	396,4	"	9,95

From this table the following conclusions may be drawn :

1. Between the equator and 45° the temperature of the ocean decreases regularly to the depth of a thousand fathoms, —beyond this no other experiments have been made.

2. The decrease of temperature is at first rapid, it gradually decreases, and becomes at last insensible.

3. The point where the decrease becomes insensible appears to rise with the latitude. At 41° and 31° it is between 200 and 300 fathoms, at 21° it is near 400. To this remark there appears to be a slight exception at 45°.53, when the temperature at 400 fathoms is still at 10° C. but perhaps that ob-

* A toise = 1.066 English fathoms.

servation is modified by the proximity of the land, since it was made in the Atlantic Ocean only 15° W. from Greenwich, and consequently near the coast of Europe, while the others were made in the south sea far from any continent; but even in this case the point where the decrease of temperature becomes insensible is still evidently near 200 fathoms.

4. The lowest temperature observed is 2.2 C. (36° F.) and it is perhaps that of all the depths at which the decrease is insensible. The locality of that temperature rises with the latitude; and it would be interesting to know at what latitude it reaches the surface.

The results of M. Lenz, in regard to the saltness of the sea, have been deduced from its specific gravity. It had previously been shown by M. Erman that salt water having a specific gravity of 1.027, the mean of that of the sea, diminishes in bulk gradually down to 25° F., and does not reach its maximum density before congelation. M. Erman's experiments on this contraction extended from 59° F. to 25°, M. Lenz extended them up to 86°, and thence deduced a law for reducing the specific gravity at any one temperature to what it would be at any other. The following table exhibits the specific gravity corrected to the temperature of 63.°5 F., distilled water at that temperature being reckoned unity.

No.	Lat. N.	Long. E.	Depth in toises.	Specific Gravity. at surface. beneath.		Difference.
1	7° 20'	21° 59'	539,0	1,02574	1,02645	—0,00070
2	21 14	196 1	665,1	1,02701	1,02666	+0,00035
"	"	"	929,4	"	1,02659	+0,00042
3	25 6	156 58	167,0	1,02706	1,02674	+0,00032
4	41 12	141 58	205,0	1,02562	1,02609	—0,00047
"	"	"	512,1	"	1,02658	—0,00096
5	32 6	136 48	214,0	1,02678	1,02624	+0,00054
"	"	"	450,2	"	1,02651	+0,00027
"	"	"	592,6	"	1,02629	+0,00049
6	32 20	42 30	1014,7	1,02825	1,02714	+0,00111
7	45 53	15 17	396,4	1,02738	1,02732	+0,00006

From this table we see that in the experiments No. 1 and 4 the specific gravity of sea water towards the bottom is a little

greater than at the surface, but that the contrary holds in Nos. 2, 3, and 5. In experiment 7 the specific gravity of the surface differs so little from that of the bottom that we may consider them as equal. For the first two cases, we may suppose that a rapid evaporation had at that time determined the slight increase of density at the surface, as abundant rains may have diminished it in experiments 2, 3, and 5. It is remarkable that in the same place the specific gravities are almost exactly the same for different depths, if we except that of the surface. No. 6 alone offers a striking exception, giving at the depth of a thousand fathoms a specific gravity much less than at the surface. We cannot suppose this difference to be due to an error of observation, the specific gravity at the bottom being the mean of three observations agreeing with each other, and that of the surface corresponding with the observations of the day before and the day after. The irregularity may perhaps be due to a current of colder and less salt water flowing at the bottom from the pole to the equator,—a point, however, which can be determined only by repeated observations. Leaving out this latter observation, we may conclude, that, *from the equator to 45° N. lat. the water of the sea to the depth of 1000 fathoms possesses the same degree of saltness.*

M. Lenz gives also two tables exhibiting the results of 258 observations made on the saltness of the sea at the surface, 105 of them made in the Atlantic Ocean, between 56°.41 S., and 50.25 N. lat., and 153 in the South Sea and Indian Ocean, between 57°.27' S. and 56°.22' N. Lat. From these tables he deduces the following results:—

1. The Atlantic Ocean is saltier than the South Sea; and the Indian Ocean, being the transition from the one to the other, is saltier towards the Atlantic on the west than towards the South Sea on the east.

2. In each of these great oceans there exists a maximum of saltness towards the north, and another towards the south,—the first is farther from the equator than the second. The minimum between these two points is a few degrees south of the equator in the Atlantic Ocean, and probably also in the South Sea, though Mr Lenz's observations do not extend to latitudes sufficiently low in the South Sea.

3. In the Atlantic Ocean the western portion is more salt than the eastern,—in the South Sea the saltness does not appear to differ with the longitude.

4. The greatest specific gravity is found in the Atlantic at the maximum point above alluded to, at 40° W. long. = 1.02856.
In the South Sea at 11.9° = 1.028084.

This last is the only one in the South Sea giving a specific gravity reaching 1.028.

5. In going north from the northern maximum, and south from the southern maximum, the specific gravity diminishes constantly as the latitude increases.

Whence then, says M. Lenz, comes these maxima towards the north and south, why is the maximum not rather at the equator? To answer this question, we must inquire what chiefly determines the saltness of the surface. Evaporation exercises the greatest influence, and by this evaporation the occurrence of these maxima may be explained. In fact, evaporation is influenced both by the heat of the sun, and by the more or less rapid motion of the currents of air. The solar heat is greatest on the equator,—but there, on the other hand, the motion of the air is least. It is remarkable that in the Atlantic Ocean the minimum coincides precisely with the locality of almost constant calms. The vapours raised by the heat of the sun remain suspended above the surface of the water, and prevent farther evaporation. The sea loses thus less of its aqueous particles, and it is consequently less salt than at 12° N. and 18° S. Lat. In these regions the trade winds carry off immediately the vapours formed by the solar heat, which is here little less than at the equator, and give place to other vapours which rise immediately. In this way evaporation proceeds, and the saltness increases rapidly. This consideration would explain also the greater saltness of the western part of the Atlantic Ocean, for we know that the more we approach the coasts of Africa, the more frequent and more continued are the calms. In the south sea great calms are not experienced towards the east, and hence the longitude has no influence on the saltness of its waters.

ART. XXI.—*Note on the Progressive Increase of Temperature as we descend beneath the surface of the Earth.*

It is long since the attention of scientific men was first directed to the observation of the high temperatures of mines, and to the natural inference it appears to suggest. The deeper the mine, the higher in general is the temperature; and data have been carefully collected, and an expression deduced from them, of the rate at which the temperature increases as we descend from the surface into the bowels of the earth. The mean rate of increase, calculated from experiments made in six of the deepest coal mines in Durham and Northumberland, is 1° F. for a descent of forty-four English feet. Cordier found it in some French mines to increase more rapidly; and the latest and apparently most carefully deduced result, that of Kupffer,* makes the temperature to increase 1° for every 36.81 English feet.

But objections of various kinds have been made to this result. Some have even refused to believe that the high temperature of mines indicates any increase of heat in the centre of the earth. They have affected to discover in the presence of many workmen,—in the candles burned,—in the gunpowder frequently employed for blasting,—and more lately † in the condensation of the air constantly rushing from the surface, sources of heat amply sufficient to raise the air and water to the temperature they are found to possess at great depths. These objections have been severally and satisfactorily answered, and the last and most ingenious of them has been ably refuted by Mr Fox,‡ who has shown, that in the Cornish mines the *ascending* has generally a *higher* temperature than the *descending* currents. The difference varied from 9° to 17° , showing that, instead of imparting heat, the currents of air actually carry off a large quantity of heat from the interior of the mines.

It has been objected also to the doctrine of a central fire, that we perceive no traces of increase of temperature in the

* *Pog. An.* xv. p. 159.

† *Edin. Review*, No. ciii.

‡ *Phil. Mag. and An.* Feb. 1830.

ocean at great depths. Now, in the consideration of this point, two principles are involved, *first*, that to which Mr Fox has adverted, that the strata at the bottom of the ocean, were they composed of solid rock the most favourable for transmitting heat, would yet propagate it much more slowly than the water which covers it, and thus all accumulation must necessarily be prevented; *second*, the principle of the maximum density of water.

All the observations hitherto made on masses of fresh water show, that at great depths * the temperature differs only a very few degrees from the point of maximum density. At greater depths it will probably be found to be very near that point. The latest experiments make the maximum density of pure water about $38^{\circ}.75$, while those of Hällstrom make it $39^{\circ}.38$. If the water be impure the point of maximum density will fall more or less, according to the nature and amount of the foreign bodies it may hold in solution. The experiments of Erman show that the point of greatest density sinks very rapidly as we add any saline ingredient.

Now, as the heaviest parts of any fluid will always find their way to the bottom, the deeper we descend in a mass of water, either salt or fresh, we must find it the colder until we come to the limit of greatest density. In fresh water lakes of great depth the temperature of the water will decrease as we descend till we reach the limit of 39° Fahr. when the temperature will undergo no farther change. In salt water the point of maximum density increases with its saltiness towards the poles,† so that the depth at which the temperature becomes stationary at about 36° F. varies from 200 to 500 fathoms. Below this there is probably no change of temperature. M. Lenz found it to sink no lower as far down as 1000 fathoms; but we can expect no *increase* of temperature without a complete subversion of the law of nature, by which a maximum of density is imparted to water.

It would appear, then, that the evidence at present is decidedly in favour of a great central heat in the globe, even leaving

* At 1000 feet Saussure found the lake of Geneva to have a temperature of 42° F.

† See this Number, page 341.

out of consideration the easy explanation it affords of so many geological phenomena. But a new source of evidence has lately been opened, and one much less liable to objections than the high temperature of mines,—in the borings for water lately practised to such an extent in France and Germany.

It was an important observation of Mr Fox, that the water which gushed out from springs at the bottom of the Cornish mines had already the temperature of the air in the places where it appeared, and was completely convincing as to the source of the heat so long observed. The Artesian springs of the continent confirm his observation. In general, the water which flows from them is of a higher temperature than the mean of the earth at the place, and is warmer as its source is deeper. At Vienna forty or fifty have been formed, and the water of all has a temperature varying from 52° to 58° , the mean temperature, according to Humboldt, being $50^{\circ}.54$ F. At Heilbronn in Wurtemberg, five borings sunk to supply a paper-mill to the depth of from 60 to 112 feet, deliver water having a temperature of about 55° ; and the proprietor has taken advantage of it to warm his works in the winter, and succeeded in keeping the apartments at a temperature of 46° when that of the air was 25° below zero of Fahr.

There are, however, many exceptions and anomalies which are not to be wondered at, when we consider, that, from the inclination of the strata and other causes, the depth of the boring is no sure indication of the true level from which the water comes. It is only when we are sure beforehand of the nature of the strata, that we can come to correct conclusions in regard to the true temperature of the earth at any given depth.

One of the most interesting of the exceptions we have met with, and which illustrates best the nature of the anomalies we may expect to meet with, is the case of a tube of $3\frac{1}{4}$ inches diameter sunk to the depth of 335 feet at the city of Tours in the basin of the Loire.* The spring ceasing to flow so freely, it became necessary to draw out the tube to within twelve feet of the surface. Immediately the water gushed out one-third more plentifully, and continued so for several hours, carrying with it a large quantity of fine sand, and many remains of

* Pogg. Ann. xxi. p. 353.

plants and shells. Among these were twigs of several inches in length blackened by the action of the water; fresh stems and roots of marsh plants,—of one species in particular so fresh, that they could not have lain more than three or four months in the water; seeds of five or six different species; and fresh-water and land shells, (*Planorbis marginatus*, *Helix rotundata* and *striata*.) All these are such remains as are found after a flood on the banks of small streams.

The water in this remarkable case, therefore, must proceed from some subterranean stream, the source of which is to be sought for at a distance among the higher grounds of Auvergne and Vivarais, and from the temperature of such sources, it is evident we can infer nothing regarding the interior temperature of the earth.

M. Magnus * has made some observations on the temperature of a boring at Rudensdorf, about five German miles from Berlin, which seem entitled to some confidence. It passes through limestone, gypsum, and sandstone, alternating with clay-slate, to a depth of sixty-five English feet. The mean temperature of the place on which no experiments have been made is assumed to differ very little from that of Berlin, 49°.1 F., and the results are as follows:

	Temper.	Diff. from mean.	For 1° F.
At 675 feet	67.66	+ 18.56	36.3 feet.
516	63.95	+ 14.85	34.7
392	62.82	13.72	28.5

The first of these results is the only one to which we can look for any approximation to the truth. And it comes very near 36.81, the result of Kupffer. The other determinations, however, are not without their value, they all indicate a more rapid increase of temperature than the truth, as we should naturally expect. For though the water as it gushes from the bottom of the tube must undergo considerable cooling on its way to the surface, yet it must still retain a considerable excess over the temperature of the strata through which it passes, and thus indicate a less distance for each degree of the thermometer, as in the results above-quoted. The same fact is also evi-

* Pogg. *An.* xxii. p. 146.

dent from the high temperature of many of these springs when they reach the surface.

M. Magnus has prefaced his paper with an account of a very ingenious maximum thermometer, with which it is highly desirable that frequent observations should be made in favourable circumstances, where borings to great depths are effected.

J.

ART. XXII.—*On the use of heated air and uncoked coal in the smelting of Iron Ores.*

THE journals have lately announced the discovery in France of a method of smelting iron ore with billets of wood uncoked, from which a great saving of expence is anticipated. This discovery will prove of high value to the iron-smelters in foreign countries, especially in the north of Europe. But to the British smelters it is vastly inferior in importance to the process now employed at the Clyde Iron Works, by which iron of an excellent quality is obtained at once, and in much larger quantity than formerly, by the employment of raw uncoked coal. The agent in this remarkable amelioration of the smelting process is heated air, with which the blast in the furnace is kept up instead of the cold air hitherto propelled into the furnace. The iron when withdrawn is much more fluid than when smelted by the old process, and in this respect has much resemblance to the Silesian iron of the first fusion. The value of this happy application in an economical point of view may be seen from the following circular drawn up by the patentee.

“Comparative view of the quantity of materials required at Clyde Iron Works to smelt a ton of foundry pig-iron, and of the quantity of foundry pig-iron smelted from each furnace weekly.

	Coals in tons of 20 cwt. each cwt. 112 lbs.	Iron- stone.	Lime- stone.	Weekly produce in pig-iron.
1. With air not heated and coke, 7 tons	7	3½	15 cwt.	45 tons.
2. With air heated and coke,	4½	3¼	10	60
3. With air heated and coals not coked,	- 2¼	3¼	7½	65

Notes.—1st, To the coals stated in the 2d and 3d lines, there will fall to be added 5 cwt. of small coals, that being required to heat the air.

2d, The expence of the apparatus for applying the heated air will be from L. 200 to L. 300 per furnace.

3d, No coals are now coked at Clyde Iron Works; at all the three furnaces the iron is smelted with coals.

4th, The three furnaces are blown by a double-powered steam engine, with a steam cylinder 40 inches in diameter, and a blowing cylinder 80 inches in diameter, which compresses the air so as to carry $2\frac{1}{2}$ lbs. per square-inch. There are two tuyeres to each furnace. The muzzles of the blowpipes are $3\frac{1}{8}$ inches in diameter.

5th, The air is heated to upwards of 600° of Fahrenheit. It will melt lead at the distance of three inches from the orifice through which it issues from the pipe.

ART. XXIII.—*On Oil of Turpentine and Artificial Camphor.*

By Dr OPPERMANN.

Oil of Turpentine

OIL of turpentine has been subjected to analysis by various chemists with nearly the same results. Those of de Saussure and Houton Labillardiere are as follows .

	De Saussure.	M. H. Labil.
Carbon,	87.788	87.6
Hydrogen,	11.646	12.3
Azote,	.566	
	100	99.9

Neither of these chemists, nor Mr Hermann of Moscow, who examined the subject more lately, found any oxygen in it. But when potassium is thrown into oil of turpentine an action takes place, and bubbles of gas are disengaged, from which the presence of oxygen might be inferred. Dr Oppermann of Strasburg has therefore subjected the oil rectified by distillation from chloride of calcium to a new analysis under the direction of M. Liebig, with the following results.

1. Carbon,	83.9828	=	31	volumes
Hydrogen,	10.6565	=	81	_____
Oxygen,	5.36	=	1	_____

100

2. Carbon,	84.5928	=	30	volumes
Hydrogen,	11.7349	=	51	_____
Oxygen,	3.6728	=	1	_____

100 *

The first of these results was obtained with the apparatus of MM. Gay-Lussac and Liebig; the latter with the improved apparatus of the latter chemist, and with a quantity six times as large as the former. It appears, therefore, the more worth of confidence, and establishes the presence of oxygen in rectified oil of turpentine.

Artificial Camphor.

Artificial camphor has been hitherto regarded as a compound of muriatic acid and oil of turpentine; consisting, according to the analysis of M. Houton Labillardiere, of,

Carbon,	76.39	=	15	vols.
Hydrogen,	9.63	=	12	_____
Muriatic acid,	14.07	=	1	_____

M. Oppermann prepared a quantity of this camphor by passing dry muriatic acid gas over rectified oil of turpentine in a long tube, and obtained a crystalline compound, which, after pressure in blotting-paper till it ceased to give out any oil, was nearly equal to one-half the weight of the oil employed. Thenard obtained 110 from 100 parts of oil, Tromsdorf, 26.6, and Cluzel 47,—a fact of great importance, as it shows that oils of turpentine differ very much in composition.

The camphor obtained by M. Oppermann was soft like wax, might be moulded between the fingers at 68°, but crumbled at 50° F. At 104° it sublimes slowly, and forms beautiful, large, and brilliant crystals. The smell is fainter than that of com-

* *Ann. de Chim.* xlvii. p. 230, and *Pogg. Ann.* xxii. p. 193.

mon camphor; the taste weak, but aromatic, and it burns with a bright flame, green towards the edges.

Dissolved in alcohol, it is not troubled by a solution of nitrate of silver, but alkalies decompose it, separating muriatic acid. Common nitric acid does not act upon it, but it is dissolved by the same acid when concentrated.

M. Oppermann obtained the chlorine by passing the camphor in vapour over quicklime heated to redness, dissolving the lime in nitric acid, and precipitating by nitrate of silver. The carbon and hydrogen were estimated by decomposing with oxide of copper. His results were,

Carbon,	72.807	=	23.8	or	24	atoms	=	291.574
Hydrogen,	9.479	=	37.97	or	38	—	=	37.974
Chlorine,	17.713	=	2		2	—		70.940
99.99 And the atomic weight = 400.488								

or it may be stated thus :

Carbon,	72.807	=	24	atoms
Hydrogen,	8.980	=	36	—
Muriatic acid,	18.212	=	2	—

The base of this compound is a carbo-hydrogen, in which the hydrogen is to the carbon as 3 : 2.

New Carbo-hydrogen.

When artificial camphor is decomposed by passing it as often as ten times over fragments of quicklime, there condenses in the receiver a clear transparent liquid oil, which becomes white and solid between 50° and 54° F. The heat of the hand melts it. Potassium undergoes no change in it, but when heated in it with contact of air, it forms a dark resinous substance. Fuming nitric and acetic acid, and caustic potash, have no action upon it, but sulphuric acid changes it into a brown resinous substance. Ether, alcohol, and the carburet of sulphur dissolve it. It possesses a very weak odour, but different from that of camphor, and a mild aromatic taste.

Analyzed by oxide of copper, he found it to consist of,

Carbon,	88.48
Hydrogen,	11.52

To prove that this was the real base of the artificial camphor, it was exposed in a liquid state to a current of muriatic acid gas, when a solid substance was obtained, having all the properties of artificial camphor.

This result explains one cause of the different properties possessed by different specimens of oil of turpentine, and why they should yield unlike quantities of artificial camphor. We have only to suppose that they hold in solution a variable quantity of the carbo-hydrogen above described, and all the anomalies will disappear.

ART. XXIV.—*On the performance of Fluid Refracting Telescopes, and on the applicability of this principle of construction to very large instruments.* By PETER BARLOW, Esq. F.R.S. Cor. Mem. Inst. of France, of the Imperial Academy of St Petersburg, &c. &c. Read December 9, 1830.

IN the *Philosophical Transactions* for 1827, a paper of mine was published containing an account of a series of experiments which I had carried on with Messrs W. and T. Gilbert on the curvature of object-glasses for telescopes. In the course of these experiments, I saw so much the difficulty which opticians experience in obtaining large pieces of good flint-glass, that I turned my attention to supplying this material by a fluid. Having, after several attempts, at length found an admirable substitute in sulphuret of carbon, I wrote a short account of my intended construction, addressed to His present Majesty, at that time Lord High Admiral, and, as such, President of the Board of Longitude, soliciting from that Board assistance in carrying forward my experiments. Having obtained this aid, the result of my first trial was the construction of an eight-inch fluid telescope, at that time the largest refractor in this country. A description of this instrument is given in the *Philosophical Transactions* for 1829, and some objects are pointed out which had been selected as tests of its performance.

I have, however, since had more time and better means of testing the instrument; *first*, through the kindness of Mr Herschel, who pointed out to me several objects that he had observed with his new twenty-inch speculum; and *secondly*,

by direct observations on the same objects in Sir James South's new twenty-foot refractor, and in my own telescope. A few of these, which serve to mark distinctly the progress I have made, are given below ; but I will first state two or three of my own observations, which, I conceive, tend also to the same object.

In the paper last referred to, I have stated my observation on η Persei, marked as double in South and Herschel's catalogue, with a small star at a greater distance ; this star is seen distinctly sextuple in my telescope. These stars I had the satisfaction of showing to M. Struve in his recent visit to England, and I have since seen them in Sir James South's telescope. Another good test of the light of my telescope is found in σ Orionis, marked in the above catalogue as two distinct sets of stars, each triple ; whereas, in my telescope, both sets are quadruple, with a double star, or rather two very fine stars between them ; the fourth star in the bright set, is a remarkably fine brilliant point, very near to the principal star, and in the same line as the nearest of the original small stars, on the opposite side, so that the three are in one line ; or more accurately, the line joining the two small stars touches the margin of the bright star. I might mention several other cases of fine double stars which I have discovered, but I select the above because it is evident that both objects have been well examined with fine instruments, and that the stars I have mentioned had, notwithstanding, escaped detection.

Of the tests furnished me by Mr Herschel I shall only select two, one of which in particular serves to point out in a very precise manner the limit of power of my telescope. This is the star β Capricorni, which, in the finder, is a coarse double star of about $3'$; but between these two stars, nearly in the middle, but a little below the line of junction, is a very fine double star, discovered by Mr Herschel, and which he considers a very severe test ; he says indeed that he requires no other, of the light of a telescope. This star I can see, and, under favourable circumstances, distinctly ; but still I have not sufficient command of it to see it double. We have thus the exact limit defined at which the light of this splendid instrument surpasses that of my telescope. The other object to

which I have alluded to is ϕ Virginis: this he considers a very easy double star, although it had before escaped detection; it is, however, rather close. This star I could see very distinctly one evening (June 4th), the moon being very bright and full, on the meridian, and within an hour of the star. I mention this object because it requires a certain degree of defining power; in point of light it involves no difficulty. Mr Herschel could see it when his aperture was reduced to six inches.

Amongst the objects which I have seen in Sir James South's twenty-feet, there is also one in particular which forms a good test of the relative power of his instrument and mine; this is Messier's twenty-second nebula. This object, which in a good $3\frac{1}{2}$ inch refractor has only the appearance of a white cloud, I saw in the above instrument resolved into an immense number of brilliant small stars. In my telescope also it is resolved into apparently as great a number of stars, but the full power of the instrument is exerted, and still the resolution seems scarcely complete. In fact, my instrument appears to labour to effect what seems to be quite within the power of the other.

I wish particularly to direct attention to this object and that of β Capricorni, because, where the same object can be seen without any very apparent difference in two instruments, or where it can be only seen in one, the great test of comparison is lost; but in those I have mentioned, the exact limit of power is defined.

Amongst the objects I have examined in Sir James South's telescope, and repeated in mine, for the purpose of comparing the defining powers of the two instruments, were the planets Jupiter and Mars. These were both more sharply defined in Sir James South's than in my telescope, but the superiority was by no means so great as I had expected. I fortunately saw the shadow of Jupiter's third satellite pass over the disc, on the 8th of August, at Kensington, and it exhibited a fine black round spot, extremely well defined; and on the 13th of the same month, I witnessed precisely the same phenomenon in mine, and as far as the definition of the shadow was concerned, with an effect in which I could not distinguish an inferiority; it had the appearance of a small black wafer on a sheet of white paper; still, however, the edge of the planet was

certainly sharper in the twenty-feet. Both evenings were amongst the finest this climate affords, and the powers employed as nearly as possible equal; viz. about 260 and 450 in both instruments. We also, at Kensington, observed Mars with various powers from 260 to 1400, and it carried 1200 well; with this the white spot near its south pole was seen beautifully distinct, as also a long dark spot on its apparent eastern limb. The bright spot at its south pole I saw also remarkably well defined in my own telescope on the 13th, and a dark spot on its disc very distinct; but it occupied the centre of its disc. The highest power, however, I used was 500, and it was probably best seen with 260.

There can be no question of the superior defining power of the twenty feet, and the light is of course also greater; still, however, when I consider that I have been comparing with two telescopes, one of twenty inches aperture, and the other of twelve inches, and each of them twenty feet in focal length, or nearly, while my telescope is barely eight inches aperture, and only twelve feet in length, I cannot but consider the comparison as highly satisfactory.

In addition to the above observations, which have been certainly highly gratifying to myself, I have also had the honour of showing the instrument to many persons, both Englishmen and foreigners, well acquainted with astronomy, and in every instance the practicability of the principle of construction has been admitted; a point by no means generally granted when the suggestion was first advanced.

Other obstacles also, independent of the arrangement of the lenses, were foreseen, which time is gradually dissipating; such as the difficulty of permanently securing the fluid, and then, admitting this to be effected, the probability of a decomposition of the glass by the fluid, &c. &c. I have, however, now the satisfaction to state, that the lens of my 3-inch telescope, filled August 5th 1827, continues in precisely its original state, no perceptible change has yet taken place in either the quantity or quality of the fluid, or in the transparency of the glass.

As far as the above observations and remarks extend, therefore, it appears that the essential properties of the flint lens are supplied by the fluid. I beg now to state a few particulars in

which the sulphuret of carbon has advantages which the glass has not :—these are, first, that in consequence of the very high dispersive power of this fluid, the correcting lens is placed so far behind the principal plate or crown lens, as to require to be only one-half as much in the diameter ; a highly important consideration in the construction of a very large telescope.

Secondly, the combination is such as to give a focal power one and a-half times the length of the tube, or, which is the same, the telescope may be reduced to two-thirds the length of a glass telescope of the usual kind, without incurring a greater amount of spherical aberration in the front lens.

Of the latter advantage, however, I have not ventured fully to avail myself in my 8-inch, because, as I knew the general opinion was against the success of the experiment, I was fearful of failing in the beginning by attempting too much.

I have therefore made the length twelve feet, to an aperture of eight inches, which, although shorter than opticians would choose to work in the usual achromatic, is not so short as this principle of construction would admit, and which in any new case I should not hesitate to adopt. Indeed, according to the form of construction I am now about to propose, a telescope of two feet aperture and twenty-four feet in length would not have more spherical aberration to contend with, than a telescope of the usual construction of six inches aperture and twelve feet length, which is fully within the range of the usual practice ; at the same time I will not undertake to say that I could on so large a scale confine the length to twelve times the aperture, although I should certainly attempt it in the first instance. But if the length extended to even fifteen or eighteen times the aperture, I have little doubt of making the instrument manageable by one person, by adequate mechanical arrangements, and of producing a telescope which would as much exceed the most powerful telescopes of the present day, as these exceed the refractors of highest repute at the close of the last century.

Whether such an instrument will be undertaken at present, depends upon circumstances which I cannot command. I can only say, that if such a construction were entrusted to my direction, no exertion should be wanted on my part, to render it

complete and worthy of the present state of English science. (See Note at the end of the article.) At all events I cannot doubt that the spirit of scientific enterprise will lead ultimately to the attempt; and in order to facilitate the accomplishment of it, as far as lies in my power, I have in the following pages described the nature of the arrangements which in my opinion would most contribute to success.

In my former paper I have given a formula expressing the relations between the length, foci, and distances of the lenses, and have remarked upon the almost infinite variety of forms to which it leads; some of them, I have stated, would probably be found in practice preferable to others, although they are all equally correct in theory. Of these cases, some have since suggested themselves to me; and others will also probably be detected, by a due examination of the formula and tables, which Professor Littrow, of Berlin, has recently presented to the Astronomical Society, relative to this form of telescope; with tables of curvatures, both direct from the formulæ of Euler (reduced to the case of open lenses), as also indirectly from principles of his own. I have not as yet had an opportunity of examining these cases, but, from the well-known ingenuity of their author, I cannot doubt of finding in this memoir many useful suggestions.

The great change, however, which I propose to make in the construction of this giant telescope, is to have two front lenses, which will be attended with advantages not involved in the above considerations. At present, in consequence of the diameter of the fluid lens being only half that of the front lens, it is difficult to get a sufficient quantity of spherical aberration in the former, to correct that of the latter;—for although we give to the plate lens the curvature requisite for reducing its aberration to a minimum, yet the fluid lens is obliged to be made considerably concavo-convex (a form not to be used when it can be avoided); in order to produce a sufficient aberration in the fluid to correct it. Moreover, I have hitherto employed parallel meniscus cheeks to contain the fluid, which present a practical difficulty, if not a positive impediment, to good centering. This will be seen immediately when we consider that when a lens is double-concave or convex, and also when it is

concavo-convex, if the radii of curvature are very unequal, the centering may always be effected: for the line joining the centres of the two spheres, or this line produced in the latter case, must pass through the lens, and indicate its true centre: but when the lens has parallel surfaces, or the radii equal, if the two spherical centres be not coincident from the tool itself (a very improbable case) the line which joins them can never cut the lens, and consequently it can have no true centre. All these evils will, however, be avoided in the proposed application of two front lenses, which, by being placed each in what opticians call their best position, will at once reduce the spherical aberration of the front lens to about one-third of its present amount, and thereby enable us to correct it by the fluid lens without adopting the distorted form rendered necessary under present circumstances.

Another important consideration is also involved in this form, relative to the facility it affords of obtaining the plate-glass. If the front lens were single, the thickness would be such as would require the glass to be made specifically for the purpose, and of course all the delay and expense of previous experiments would be incurred; whereas, by dividing the whole amount of curvature between the two, the usual thickness of plate-glass, as at present manufactured, would be sufficient, and we might have the selection from large stores of the best glass at a trifling expense; and as to that of the correcting fluid, or substitute for the flint-glass, it is so very inconsiderable as not to deserve being mentioned; although, if it were possible to obtain a piece of flint-glass large enough for such a purpose, scarcely any price, however great, would be thought exorbitant. In the instrument proposed, nearly the whole expense would be the workmanship, and I must think it very inconsiderable in comparison with the magnitude and importance of the undertaking.

I had intended to have concluded this paper by giving the curvature, foci, &c. which I have computed; but as they are merely suppositions, as far as they are dependent on the index and dispersion of the front glass, it is perhaps better to withhold them. The only object I had in making them was to form some idea of the requisite curvature, thickness of glass,

&c. They can only of course be permanently made after the plate-glass has been selected.

NOTE BY THE EDITOR.

In consequence of a representation made to the Council of the Royal Society respecting the construction of a telescope on Mr Barlow's principle of great dimensions, a committee of inquiry was appointed, and having received from them a favourable report, the Council have given orders to Mr Dollond to execute a telescope of that description under the superintendence of Mr Barlow. The members of the Royal Society, and men of science of every country, will, we are sure, join with us in thanking the Council for this act of true liberality to science. This is the legitimate mode of applying the funds of a great institution, and we hope to see it extensively imitated by all our provincial societies. The chance of having an important invention submitted to a fair trial, when such a trial is beyond the means of an individual, is one of the best excitements to inventive genius; while the actual introduction of new and valuable inventions, which must to a certain extent be the consequence of this patronage, cannot fail to be useful to the trade of the country. Government would, we are confident, willingly assist so distinguished a body as the Royal Society in bringing this part of their plan into wider operation.

ART. XXV.—*First Report of the Proceedings, Recommendations, and Transactions of the British Association for the Advancement of Science.* Printed by order of the General Committee. 8vo. Pp. 112. York, 1832.

THE appearance of this Report has put the scientific public in possession of the authorized account of what has been done by this very important society, and of what may be expected from its exertions.

We cannot but consider that the British Association has started under auspices the most favourable, and even the results of the first meeting, and the prospects of support which this Report displays, certainly exhibit a much greater step towards

the fulfilment of a great series of objects requiring much time for their developement than could possibly have been anticipated.

That the Association has already secured in its favour a great share of the best talent in the country, the list of members annexed to the Report will best testify ; it is in every respect a most creditable phalanx of names. Fortunately, however, it is not upon names alone that its prospects of support must rest ; the pamphlet before us contains pledges of exertion from some of the most active philosophers in the country, of a description which has been unattempted by any previously existing society among the many which this country possesses. There is no society strictly scientific which professes to direct the attention of its members, or of philosophers at large, to particular branches of knowledge, in which important progress might be made by a well directed system of mutual co-operation. Take, for example, the science of meteorology, in which observers have been so long at cross-purposes, and in which the very same amount of exertion, which has been thrown away upon forming registers utterly useless for want of information or combined exertion, might have been rendered available to the highest purposes of science. To this subject the Association has properly directed peculiar attention, and, it may be hoped, with the happiest results.

But there is nothing which strikes us with more admiration amongst the energetic proceedings of this infant institution than the list of reports on the state and progress of the different sciences. This is a species of literature quite new in this country, and which will form a body of information really invaluable. The only approximation to it has been in the annual addresses of the President of the Geological Society upon that particular branch of science. These admirable discourses have only excited the wish that something more general, upon the same plan, should be attempted. The objects of these scientific reports will, we believe, be more varied and extended ; and it is probable, that, for the first year at least, the style of execution of them will much depend upon the views and opinions of the individuals by whom they are composed,—until some normal plan shall have been sanctioned by usage for

general adoption. Some account of the different labours of philosophers during a recent period of time, with a view of their bearing upon the past progress and future prospects of the sciences, will, we presume, be a general object of these reports; but we trust that in many of them more extended and philosophical generalizations will be taken; and in several sciences we do not know a more acceptable service that could be performed, than a correct estimate of the precise point to which our knowledge has arrived, and where precisely our ignorance begins. From this process some conclusion will naturally be drawn as to the line or lines which present the fairest points of attack upon the remaining strong-holds of science.

The names of those who have engaged to execute these useful labours in science include some of the most active philosophers in Britain; and we anxiously hope that nothing will deter those who have given this capital portion of the machinery of the Association the weight and sanction of their names, from fulfilling, in as full a manner at least as leisure will permit, the agreement to which they are in some measure pledged, and which will be valuable to science and to the Association, in proportion to the acknowledged eminence of their scientific character.

As we cannot but look upon this as one of the most important parts of the pamphlet before us, we shall insert the list of reports contained in the preface.

“ 1. The Rev. George Peacock has undertaken to present to the next Meeting, a Report on the recent progress of Mathematical Analysis, in reference particularly to the differential and integral calculus.

“ 2. Professor Airy has undertaken a Report on the state and progress of Astronomical Science, in reference particularly to Physical Astronomy.

“ 3. J. W. Lubbock, Esq. has consented to furnish such information respecting the data and desiderata for calculating the time and height of High-water as he may be able to offer.

“ 4. James D. Forbes, Esq. has undertaken to present a Report on the present state of Meteorological Science.

“ 5. Dr Brewster has undertaken a Report on the progress of Optical Science.

“ 6. The Rev. Robert Willis has undertaken a Report on the state of our knowledge concerning the Phenomena of Sound, in reference especially to the additions recently made to it.

“ 7. The Rev. Professor Powell has undertaken a similar Report respecting the Phenomena of Heat.

8. The Rev. Professor Cumming has undertaken a Report on Thermo-Electricity and on the allied subjects, in reference to the discoveries recently made in them.

“ 9. James F. W. Johnston, Esq. has undertaken a Report on the recent progress of Chemical Science, especially in foreign countries.

“ 10. The Rev. Professor Whewell has undertaken a Report on the state and progress of Mineralogical Science.

“ 11. Robert Stevenson, Esq. has undertaken the Report recommended by the Geological and Geographical Committee, on the waste and extension of the land on the east coast of Britain, and on the question of the permanence of the relative level of the sea and land.

“ 12. Professor Lindley has undertaken to give an account of the principal questions recently settled, or still agitated, in the Philosophy of Botany.” Pp. iii.—v.

No less than six of these twelve reports are to be contributed by eminent members of the University of Cambridge, —a circumstance certainly highly indicative of the warmth of feeling of that great body towards the objects of the British Association, as well as giving a pledge for the ability of the works thus undertaken. The former distinguished professor of mineralogy at Cambridge was among the earliest supporters of the plan, to which he alluded in a letter communicated to the York Meeting, an extract from which is given in Mr Vernon Harcourt's address. “ A collection of reports,” he says, “ concerning the present state of science, is on all accounts much wanted ; in order that scientific students may know where to begin their labours, and in order that those who pursue one branch of science may know how to communicate with an inquirer in another. For want of this knowledge we perpetually find speculations published which show the greatest ignorance of what has been done and written on the subjects to which

they refer, and which must give a very unfavourable impression of our acquirements to well-informed foreigners."—Report, p. 29.

The Report is divided into three distinct portions, as the title-page indicates. The first contains the Proceedings of the Meeting towards the constitution of the Permanent Body or Association which sprung from it, and the subsequent arrangements necessary to its complete constitution. The second part embraces the Proceedings of the general and the sub-committees appointed to carry into effect the objects of the Association, and which, it will be seen, were by no means limited to the week of the meeting at York. The third division contains the Scientific Transactions of the meeting at York, comprising the order and periods of the meetings, with abstracts of the papers presented. One communication, printed at full length, is the excellent Essay of Dr Henry of Manchester, upon the philosophical character of Dr Priestley, reprinted in our present number.

It is not our intention to enter into any analysis of these several departments. The general outlines of the first and third are indeed already given in Mr Johnston's account of the meeting in the last number of this *Journal*, which likewise contained the statement of the objects and rules of the Association, and lists of the committees. But we feel it to be only due to the very able address of Mr Vernon Harcourt, the real originator of the *permanent* association, to make one or two quotations from it; for we view it as an address differing much from what is too often found upon such occasions, as perfectly free from affectation, as abounding in the soundest views of the present state of science, and written in a manly and highly perspicuous style.

The following passage sets in a true light the bearing of the new association upon already existing societies:—"It is not by counting the great luminaries who may chance to shine in this year, or that,—in a decade of years, or a generation of men,—that we are to inform ourselves of the state of national science. Let us look rather to the numbers engaged, effectually, though less conspicuously, in adding by degrees to our knowledge of nature; let us look to the increase of scientific

transactions and journals; let us look, Gentlemen, at the list produced this day of philosophical societies which have grown up in all parts of the kingdom. The multiplication of these new and numerous institutions indicates a wide extension of scientific pursuits. The funds so liberally contributed to their support bear evidence of an enlarged disposition in the public to promote such pursuits.

“ It is on this very ground I rest the necessity and the practicability of establishing in science a new impulsive and directive force, that there are new and more abundant materials to be directed and impelled. The mining-field of discovery seems to me to show, on the one part, the ore breaking out on every side; veins of the precious metal scantely opened or imperfectly wrought; and on the other a multitude of hands ready to work it; but no one engaging them to labour, or showing them in what manner they may employ their industry to the best advantage. And therefore it is that I propose to you to found an Association including all the scientific strength of Great Britain, which shall employ a short period of every year in pointing out the lines of direction in which the researches of science should move, in indicating the particulars which most immediately demand investigation, in stating problems to be solved and data to be fixed, in assigning to every class of mind a definite task, and suggesting to its members, that there is here a shore of which the soundings should be more accurately taken, and there a line of coast along which a voyage of discovery should be made.

“ I am not aware, Gentlemen, that in executing such a plan we should intrude upon the province of any other institution. There is no society at present existing among us, which undertakes to lend any guidance to the individual efforts of its members, and there is none perhaps which can undertake it. Consider the difference, Gentlemen, between the limited circle of any of our scientific councils, or even the Annual Meetings of our societies, and a meeting at which all the science of these kingdoms should be convened, which should be attended, as this first meeting you see already promises, by deputations from every other society, and in which foreign talent and character should be tempted to mingle with our own. With what a mo-

mentum would such an Association urge on its purpose ! what activity would it be capable of exciting ! how powerfully would it attract and stimulate those minds, which either thirst for re-putation or rejoice in the light and sunshine of truth !” Pp. 11—18.

The same views are perhaps still more happily presented in the following paragraphs,—which we really hope will go far to do away with the erroneous impression, still existing in some quarters, of an intentional interference on the part of the new institution with other scientific bodies.

“ But there is a defect in these separate societies, in respect to their own immediate objects, which I am sure no member of them would wish to dissemble, and which arises from the narrow basis on which they are of necessity built. It is not only, that the constant converse of men, who, to borrow the expression of Goldsmith, have often travelled over each other’s minds, is not half so effectual in striking out great and unexpected lights, as the occasional intercourse of those who have studied nature at a distance from each other, under various circumstances and in different views ; but it is also, Gentlemen, that none of our existing societies is able to concentrate the scattered forces even of its own science ; they do not know, much less can they connect or employ, that extensive and growing body of humble labourers who are ready, whenever they shall be called upon, to render their assistance.” P. 20.

“ What numberless suggestions, what a crowd of valuable but abortive hints are continually floating in the thoughts of philosophers, for the pursuit of which time is wanting to themselves ! Now I say, Gentlemen, that we have among us, scattered through the country, men willing to adopt these unexecuted hints, as they arise out of the profound and varied meditations of more experienced minds, men not incapable of surveying with accuracy a limited district, though they may not pretend to draw the general outline of the map, or fill up the whole of its details. Many such there are who only wait for instructions, and who require no other stimulus than that of being invited, to render the most essential service to researches and calculations of the highest order ; and it is upon this ground especially that we venture to pronounce an Institution wanting, which shall not hesitate to make such invitations and to

offer such instructions ; it is upon this ground that if we now propose to revive in the nineteenth century a plan devised two centuries ago,—we see a difference, Gentlemen, in the probability of success. Scientific knowledge has of late years been more largely infused into the education of every class of society, and the time seems to be arrived for taking advantage of the intellectual improvement of the nation. Let Philosophy at length come forth and show herself in public ; let her hold her court in different parts of her dominions ; and you will see her surrounded by loyal retainers, who will derive new light and zeal from her presence, and contribute to extend her power on every side.

“ Much, indeed, is not to be gained in the more recondite subjects of investigation from the first essays of inexpert inquirers ; but let the number of those inquirers only be increased, collect around you, Gentlemen, a school fired with a zeal for truth, confess to them how little you know compared with what remains to be known, apprise them that there is not a subject to which they can apply themselves where new materials are not wanted to advance the fabric or secure the foundations, let them see that the more multiplied have been your discoveries, the more additional openings to discovery have appeared,—and if you will then draw the precise line of what is, and what is not made out in every science, if you will indicate to them those promising points and *inlets* of inquiry which bid fair to lead to promising results,—if you will thus put before them right subjects, and at the same time suggest the right methods of treating those subjects ; whatever more may be wanting to accurate and successful investigation, natural sagacity and a longer experience will easily supply to men possessing only common abilities, and walking in the common paths of life.” Pp. 20—22.

We feel that we should discharge but imperfectly our duty in disseminating as widely as possible the objects of this Report, did we not transfer to our pages the recommendations of the sub-committees of the Association upon different branches of science, who are in fact the active organs of the Association, forwarding its scientific objects, and providing the intellectual occupation of its successive meetings.

“ RECOMMENDATIONS OF THE SUB-COMMITTEES.

“ *Committee of Mathematical and Physical Science.*

“ *Mathematics.*—The Committee recommended that the Vice-President of the Association residing at Cambridge be requested to use his utmost efforts to procure, from some competent individual, a Report to the next Meeting on the progress of Mathematical Science.

“ *Astronomy.*—That Professor Airy be requested to favour the Association with a Report on the state and progress of Physical Astronomy, together with such remarks on the improvements of Practical Astronomy, as he may deem it useful to add.

“ *Theory of Tides.*—That J. W. Lubbock, Esq. be requested to furnish a statement of the means which we possess, or which we want, for forming accurate tables for calculating the time and height of High-Water at a given place.

“ *Meteorology.*—That James D. Forbes, Esq. be requested to draw up a Report for the next Meeting, on the present state of Meteorological Science.

“ The Committee, considering that the science of Meteorology is in more want than, perhaps any other, of that systematic direction which it is one great object of the Association to give, has thought it advisable to propose the following points for investigation.

“ I. That the Association should employ all the means in its power to procure a *Register of the Thermometer during every hour of the day and night, to be kept at some military or naval station in the South of England.*

“ *Note.** Until the phenomena and distribution of diurnal temperature are more thoroughly understood than at present, we can hardly hope that any very sure footing has been obtained in the study of meteorology. The hourly register kept for several years at the military station of Leith Fort in lat. 56°, has shown that we want nothing but the combination of a sufficient number of trustworthy observations, in order to obtain results of primary importance to the science, and which may one day enable us to arrive at the true form of the daily

* The notes appended to the Recommendations have been drawn up by some of the Members of the Committees since the Meeting.

and annual curves of mean temperature with a precision almost mathematical. In order, however, to extend the benefit of such investigations, it is absolutely necessary that they should be pursued in different latitudes. The application to rendering available registers otherwise almost without value, from not being made at the proper hours, will be best illustrated by a reference to the account of the Leith observations. (*Transactions of the Royal Society of Edinburgh*, vol. x.)

“ II. That the establishment of such an *hourly meteorological register* be pointed out as a highly interesting object, in reference especially to the important point of *intertropical climate*, to THE COMMITTEE OF THE ASSOCIATION IN INDIA.

“ III. That the Committee in India be requested to endeavour to institute such observations as may throw light on the *phenomena of the horary oscillations of the barometer near the equator*. Should the concurrence of the Committee on these points be obtained, it would probably be desirable that the Association should take measures for sending out delicate and accurate instruments.

“ IV. That Mr Phillips and Mr Wm. Gray, jun. of York, be requested to undertake a *series of experiments on the comparative quantities of rain falling on the top of the great tower of York Minster, and on the ground near its base*. The Committee has been induced to propose this specific question in consequence of the local fitness of the situation, and the facilities offered for its solution by the authorities; but it is to be wished that similar experiments should be made elsewhere, that by an extended comparison of observations, light may be thrown upon the anomalies which have been observed at Paris and in other places.

“ V. That the Association should express its desire to receive a *satisfactory exposition of the theory of the moistened bulb hygrometer*, and that observers be also invited to institute *series of comparative experiments on the indications of the moistened thermometer and the temperature of the dew point*.

“ *Note.* These indications may be ascertained by Mr Dalton's process, or by Mr Daniell's hygrometer, or by both. Notwithstanding the ingenious and laborious researches of Hutton, De Saussure, Leslie, Anderson, and Gay-Lussac upon this subject, scientific deductions drawn from more extended expe-

riments are greatly wanted. The simplicity and certainty of the experiment by which the cold produced by the evaporation of water is measured, renders an accurate theory of the result peculiarly desirable. The experimenter would do well to consult Mr Dalton's views on the theory of Hygrometry, contained in his *Meteorological Essays*, and in the *Manchester Transactions*, and to examine the investigations of Professor Leslie, (*Relations of Heat and Moisture, and Supplement to the Encyclopædia Britannica, Article METEOROLOGY*;) of Dr Anderson, (*Edinburgh Encyclopædia, Article HYGROMETRY*;) and of M. Gay-Lussac, (*Biot, Traité de Physique, tom. ii.*) A good series of observations at high temperatures will be found recorded in Nos. II. and III. of a Calcutta Journal, entitled, *Gleanings in Science*.

“VI. That experiments on the *Decrease of Temperature at increasing heights in the Atmosphere* be recommended as an important subject for the contributions of observers.

“*Note.* Series of observations for considerable periods of time on the mean temperature of the air at fixed hours, and at stations of which the difference of height has been accurately measured, are the most valuable. The best hours for observation are those which give most accurately the mean temperature of the period of observation. The hourly observations at Leith Fort have determined the hours which give the annual mean temperature in this country to be about 9½ A. M. and 8½ P. M. Experimental balloons have lately been employed to assist the solution of this problem, which is one of the most interesting in meteorology, but the investigation of it is nearly brought to a stand for want of sufficiently numerous observations. The observer may be referred for information to Ramond, *Mémoires sur la formule Barometrique de la Mécanique Celeste*; to the Researches of Humboldt; to Professor Leslie, *Supplement to the Encyclopædia Britannica, Article Climate*; to Pouillet, *Elemens de Physique*; to Mr Atkinson's Paper on Refractions in the *Memoirs of the Astronomical Society*; and to Mr Ivory's Memoir on the same subject in the *Philosophical Transactions*, and his Papers in the *Annals of Philosophy*.

“VII. That the observation of the *Temperature of Springs at different heights and depths* should be pointed out as an object of great interest, in prosecuting which insulated inquirers may render essential aid to science.

“ *Note.* When springs are copious, a few observations in the course of the year, suffice to give with great accuracy their mean temperature. The height of the springs above the mean level of the sea, and the depth of Artesian wells should be carefully observed, and where the corresponding mean temperature of the air can be obtained, it should be stated. In two points of view these observations are important, independently of the inferences which they may furnish as to the decrease of heat in the atmosphere. The great interest attached to the phenomenon of the progressive increase of temperature of the globe, as we descend through the strata, renders of value observations on the temperature of springs at considerable heights, of springs in mines, and of those brought to the surface from some depths by the process of boring. This question has been treated with great success by M. Cordier, in several Memoirs, some of which have been translated into English. Again, the researches of Humboldt, Buch, Wahlenberg, and most recently Kupffer in a Memoir on *Isogeothermal Lines*, read before the Academy of St Petersburg, in 1829, have shown that the temperature of the earth differs in many parts of the globe from that of the air, being generally in defect below lat. 56°, and in excess beyond it. The progressive increase of temperature with that of the depth in Artesian wells, and the deviation of the mean temperature of the Earth from that of the Air in different latitudes, have opened new fields for discussion; and by the zealous co-operation of observers cannot fail to present results of which, at present, we can form but an imperfect idea.

“ *Magnetism.*—It appears to the Committee highly desirable that a series of observations upon the *Intensity of Terrestrial Magnetism in various parts of England* be made by some competent individual, similar to those which have recently been carried on in Scotland by Mr Dunlop.

“ Should the Committee succeed in finding some individual ready to undertake the task, they propose that an application should be made to the Royal Society of Edinburgh, for permission to make use of the Standard Needle belonging to them, and constructed under the direction of Professor Hansteen of Christiania.

“ It appears to the Committee of considerable importance,

that a certain number of observations should be made throughout Britain with the *Dipping Needle*, in order to reduce the Horizontal to the true Magnetic Intensity.

“ *Note.* The time of three hundred vibrations should be observed, and the methods of observation and reduction should be the same as have been employed and described by Humboldt, Hansteen, and others.

“ *Electro-Magnetism.*—The Committee recommend as an important subject for farther prosecution, the examination of the *Electro-Magnetic condition of Metalliferous Veins*. The Committee would refer for the details of what has been already done upon this subject, to the Paper of Mr Fox in the *Philosophical Transactions* for 1830, and would propose that the experiments should be extended to veins which traverse, as in some of our mines, horizontal and dissimilar strata.

“ *Optics.*—That Dr Brewster be requested to prepare for the next Meeting, a Report on the progress of Optical Science.

“ *Acoustics.*—That the Rev. Robert Willis be requested to prepare for the next Meeting, a Report on the state of our knowledge concerning the phenomena of Sound, and the additions which have been recently made to it.

“ *Heat.*—That Professor Powell be requested to prepare for the next Meeting a similar Report respecting heat.

“ *Electricity.*—That Professor Cumming be requested to prepare for the next Meeting a similar Report on Thermo-Electricity, and the allied subjects in which recent discoveries have been made.

“ *CHEMICAL COMMITTEE.*—It appears to the Committee of supreme importance, that Chemists should be enabled, by the most accurate experiments, to agree in the *relative weights of the several elements, Hydrogen, Oxygen, and Azote*, or, which amounts to the same thing, that the specific gravity of the three gases should be ascertained in such a way as would insure the reasonable assent of all competent and unprejudiced judges.

“ They think it highly desirable that the doubts which remain respecting the *proportions of Azote, Oxygen, &c. in the atmosphere* should be removed; that the *proportions of Azote and Oxygen in nitrous gas and nitrous oxide* should be strictly

determined; and that *the specific gravities of the compound gases in general* should be more accurately investigated.

“ They recommend that the members of this Committee, and British Chemists in general, be invited to make experiments on these subjects, and communicate their results to the next Meeting at Oxford.

“ That Mr Johnston be requested to present to the next Meeting a view of the recent progress of Chemical science, especially in foreign countries.

“ That Dr Daubeny be requested to undertake an investigation into the sources from which organic bodies derive their fixed principles.

“ That Mr Johnston be requested to undertake the inquiries which have been suggested to the Committee into the comparative analysis of iron in the different stages of its manufacture.

“ That Mr West be requested to pursue the experiments contemplated by him into the combinations of gaseous bodies when passed through heated tubes.

“ That the Rev. W. Vernon Harcourt be requested to prosecute the inquiries contemplated by him into the chemical phenomena from which the materiality of what are sometimes called etherial substances has been inferred.

“ MINERALOGICAL COMMITTEE.—The Committee recommend that the Rev. Professor Whewell be requested to present to the next Meeting a report on the state and progress of Mineralogy.

“ GEOLOGICAL AND GEOGRAPHICAL COMMITTEE.—The Committee recommend that geologists be requested to examine strictly into the truth of that part of the theory of M. Elie de Beaumont, in its application to England, Scotland, and Ireland, which asserts that *the lines of disturbance of the strata assignable to the same age are parallel*, and that a report to the next meeting on this subject should be procured.

That Mr Phillips be requested to draw up, with such co-operation as he may procure, *a systematic catalogue of all the organized fossils of Great Britain and Ireland*, hitherto described, with such new species as he may have an opportunity of accurately examining, with notices of their localities and geological relations.

“ The Committee propose that Mr Robert Stevenson, Civil Engineer, be requested to prepare a report upon the *waste and extension of the land on the East coast of Britain, and the question of the permanence of the relative level of the sea and land*; and that individuals who can furnish observations, be requested to correspond with him on the subject. *

“ The importance which, especially of late years, has been attached to facts of this nature, in illustration of the sciences of hydrography and geology, and the mass of uncombined materials which have recently been accumulating, have induced the Committee to make the present recommendation; and in doing so it feels pleasure in being able to have in its view an individual whose practical acquaintance with the coast in general, and more particularly the minute survey made by him some years since, gives reason to expect from his report much important and accurate information.

“ **BOTANICAL COMMITTEE.**—The Committee recommend that Professor Lindley be requested to prepare for the next meeting an account of the principal questions recently settled, or at present agitated, in the philosophy of Botany, whether in this country or abroad.

“ That Botanists in all parts of Great Britain and Ireland be invited to compose and communicate to the meetings of the Association, catalogues of *county or other local Floras*, with *indications* of those species which have been *recently introduced*, of those which are *rare or very local*, and of those which *thrive, or which have become or are becoming extinct*, with such remarks as may be useful towards determining the connection which there may be between the *habitats* of particular plants and *the nature of the soil and the strata* upon which they grow; with statements of the *mean winter and summer temperature* of the air and water at the highest as well as the lowest elevation at which species occur, the *hygrometrical* condition of the air, and any other information of an historical, economical, and philosophical nature.

“ *Note.* If upon this plan a complete botanical survey of the British islands could be obtained, the results would be important when the Flora in the aggregate came to be compared with its relations of soil, climate, elevation, &c.” Pp. 47—55.

* Communications may be addressed to Robert Stevenson, Esq. Engineer to the Northern Lighthouse Board, Edinburgh.

To those not accustomed to take a part in the co-operation of general plans, all this machinery may appear very simple. Such, however, as have had any experience in such matters will be aware, that nothing short of great zeal and perseverance could have accomplished the task of putting so many distinct trains of wheels in motion, of regulating the amount of action in each, and of directing the result of the whole to one common object. The very correspondence which this undertaking demanded was obviously one of no common labour, and, as nothing occurs in the report to indicate by whom so much time and exertion was gratuitously spent upon this great scientific object, and by whom the report itself, drawn up with such uncommon care, has been produced, it is but justice to state, that, for this the Association has been entirely indebted to the York Committee, and most especially to the Reverend William Vernon Harcourt, and to Mr Phillips, Secretary of the Yorkshire Philosophical Society. It will require all the zeal of the active individuals of those places where the future meetings of the Association are to be held, to follow with effect the example thus set by the gentlemen of Yorkshire.

The prospects of the meeting at Oxford, which is to commence on the 18th of June of the present year, exceed even sanguine expectation. We hope that not a single society of any name will be unrepresented on that occasion.

ART. XXVI.—*Summary of Meteorological Observations made at Kendal in December 1831.* By Mr SAMUEL MARSHALL.
Communicated by the Author.

State of the Barometer, Thermometer, &c. in Kendal in December 1831.

Barometer.		Inches.
Maximum on the 28th,	- -	30.27
Minimum on the 7th,	- -	28.55
Mean height,	- -	29.47
Thermometer.		
Maximum on the 12th,	- -	52°
Minimum on the 29th,	- -	29°
Mean height,	- -	41.68°
Quantity of rain, 4.980 inches.		
Number of rainy days, 21.		
Prevailing wind, west.		

The greater part of this month has been very wet, and the ground saturated with moisture. The atmosphere has been very foggy. The barometer has been very variable, and we have had very little frost. There has been great uniformity in the weather, and nothing at all striking to remark. The Aurora Borealis has not been observed during the month.

ART. XXVII.—REGISTER OF THE BAROMETER, THERMOMETER, AND RAIN-GAGE, kept at Edinburgh. By ALEX. ADIE, Esq. F. R. S. Edin.

THE Observations contained in the following Register were made behind the Regent Terrace, on the south-east slope of the Calton Hill, by means of very nice instruments, constructed by Mr Adie. The height of the instruments is 246 feet above the medium level of the sea. The morning and evening observations were made about 10 A.M. and 10 P.M.

DECEMBER 1831.										
D. of Week.	Day of Month.	Thermometer.			Register Therm.			Barometer.		Rain.
		Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.	
T.	1	45	42	43.5	41	46	43.5	29.98	29.88	
F.	2	45	43	44	38	47	42.5	29.87	29.93	
S.	3	45	44	44.5	39	45	42	29.87	29.78	
S.	4	47	42	44.5	40	47	43.5	29.70	29.48	.02
M.	5	50	46	48	39	51	45	29.35	29.08	
T.	6	45	42	43.5	41	47	44	29.02	28.87	.25
W.	7	47	44	45.5	39	47	43	28.41	28.60	.32
T.	8	44	43	43.5	40	47	43.5	28.87	28.87	.10
F.	9	48	46	47	40	50	45	28.70	28.93	
S.	10	47	42	44.5	39	51	45	29.08	29.22	
S.	11	47	46	46.5	40	54	47	28.85	28.96	
M.	12	45	46	45.5	40	53	46.5	28.95	28.36	.19
T.	13	45	43	44	43	49	46	28.83	29.21	
W.	14	42	38	40	38	45	41.5	29.27	29.35	
T.	15	45	38	41.5	37	46	41.5	29.20	29.32	
F.	16	42	41	41.5	36	45	40.5	29.25	28.96	
S.	17	44	46	45	38	46	42	29.22	28.83	
S.	18	44	44	44	40	47	43.5	28.82	28.83	
M.	19	45	40	42.5	40	47	43.5	29.08	29.33	
T.	20	38	40	39	36	44	40	29.35	29.20	
W.	21	42	37	39.5	37	45	41	29.35	29.47	
T.	22	42	40	41	35	45	40	29.18	29.22	
F.	23	40	41	40.5	36	46	41	29.65	29.88	
S.	24	46	47	46.5	37	48	42.5	29.97	29.97	
S.	25	49	45	47	43	50	46.5	29.83	29.83	.38
M.	26	42	35	38.5	40	45	42.5	30.12	30.32	
T.	27	37	33	35	32	41	36.5	30.37	30.33	
W.	28	32	35	33.5	29	35	32	30.38	30.30	
T.	29	37	38	37.5	32	39	35.5	30.22	30.23	
F.	30	35	39	37	32	39	35.5	30.15	30.10	
S.	31	37	38	37.5	32	39	35.5	30.13	30.03	
Sum.		1339	1284	1311.5	1169	1426	1297.5	913.02	912.67	1.26
Mean.		43.19	41.42	42.30	37.71	46	41.85	29.452	29.441	

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Fig. 1.

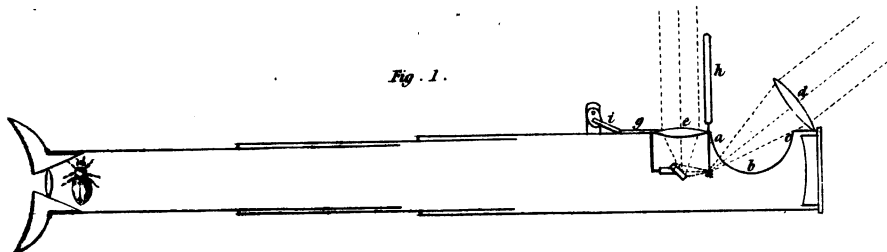


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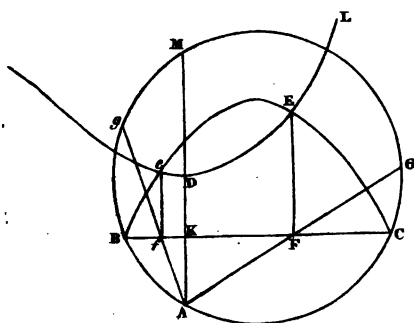


Fig. 2.



Fig. 4.

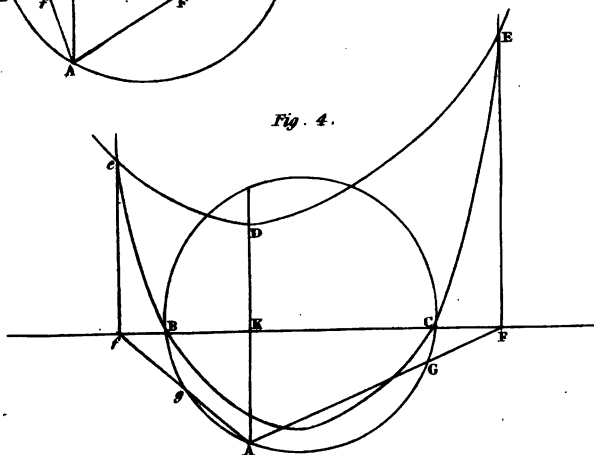


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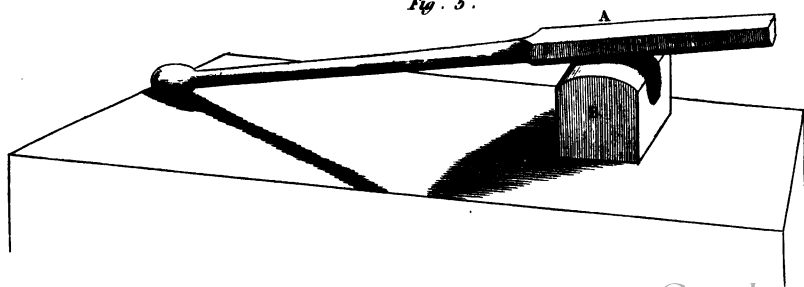


Fig. 1.

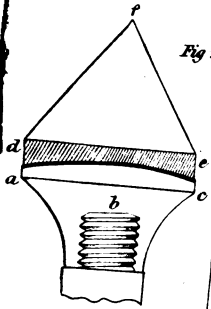


Fig. 3.

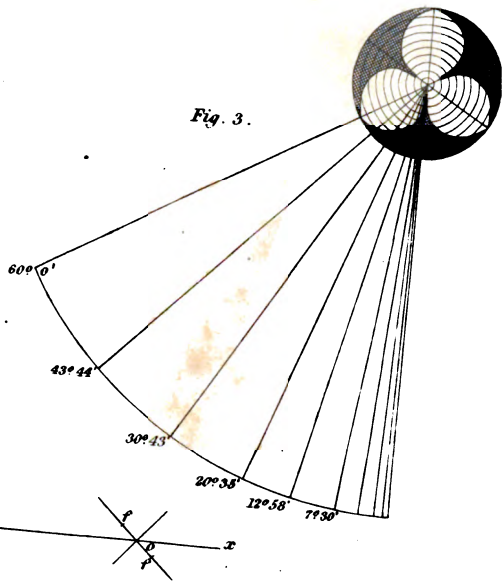


Fig. 2.

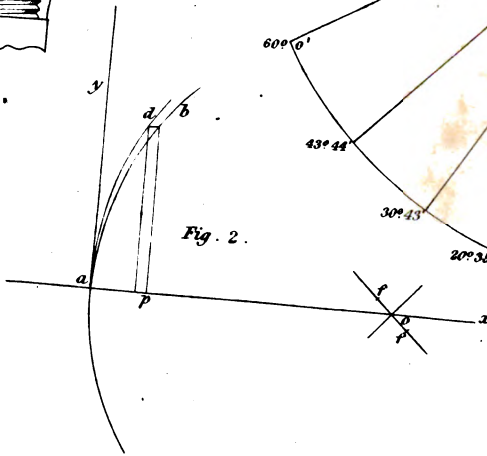


Fig. 4.

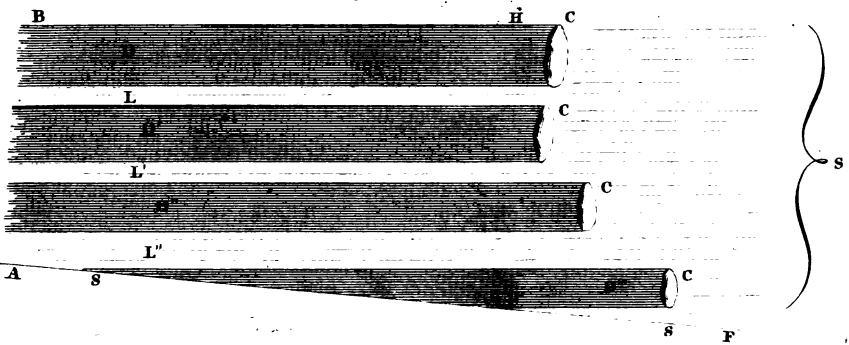


Fig. 5.

